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COURSE OF

LECTURES ON THE BLOOD,

AND ON

THE CHANGES WHICH IT UNDERGOES DURING DISEASE,

Delivered at the College of France in 1837-8,

BY

M. MAGENDIE.

LECTURE VII.

Effects of pressure on the parietes of the blood-vessels.—Considerations on transfusion.—Action of pain in increasing the pressure.—Effects of galvanism.—Nature of the buffy coat of the blood.—Differences between the fluid in and out of the body.—The fibrine of the blood different from that of muscle.—Effects of bloodletting.

GENTLEMEN:—If you take a caoutchouc tube, and force into its cavity more liquid than is required to fill it accurately, its walls yield, and are dilated. In proportion as you add fluid, the distension increases; in other words, the interior pressure augments in the direct ratio of the volume of the injected liquid. Now, it appears to me quite natural, that the arteries should be subjected to the same physical law; nevertheless, this is a view which, I admit, stands in much need of confirmation. I have already attempted, in your presence, to increase the pressure, by adding to the mass of the blood; but the mercury, instead of rising in the trials I made, underwent a considerable fall. It seems probable that the opposition between the anticipated and real results of the experiment, is to be attributed to the nature of the liquid injected. The volume of the blood was, no doubt, rendered more considerable than before, but the energy of the heart's contractility was diminished by the presence of water. My injection had the same effect as the diluents used in practice, for the purpose of moderating the violence of fever; the augmentation of the aqueous part of the blood weakened the force of impulsion of the left ventricle, and the pressure in consequence became less than previously through the entire system. I propose resuming this experiment to-day, but in a modified form; instead of introducing water, or any other debilitating fluid, into the circulation, I will make use of blood drawn from an animal of the same species as that on which our experiment is made; in fact, we will practise transfusion; and it affords me some satisfaction to have an opportunity of performing that operation before you,—one which enjoyed, in former times, the highest degree of reputation, and which has now fallen into the most complete discredit. Nevertheless, circumstances may arise wherein it would be proper to have recourse to it: I have myself many times injected medicinal fluids directly into the veins of the living subject. At the period of its discovery the operation of transfusion was received with a degree of enthusiasm bordering on frenzy. Some imprudent essays, however, made on our own species, were followed by the most disastrous results, and then the new discovery, on which a multitude of flattering illusions had been founded, was abandoned with as much inconsiderateness as it had been practically adopted.

I am of opinion that there was too much hastiness shown, both in the acceptance and in the subsequent total rejection of this last resource of medicine. It would be indispensably necessary, before introducing the transfusion of blood into the regular practice of surgery, to submit its performance to strict rules, due attention being also paid to the physical properties and composition of the liquid employed. The globules have not the same volume, or form, in different animals; the degree of coagulability of their blood is not uniform. If the blood be introduced by one process the circulation continues free; if by another, its continuance will become physically impossible. Here, as in every other experimental essay, it is right to study the results of the operation in animals, before we venture to apply it to man.

There are two principal methods of performing transfusion; in the one the blood, if

allowed to pass immediately from the vessels of the animal that supplies, into that of the animal intended to receive it; in the other it is first collected in a vase of some sort, and then injected. The latter method is more generally adopted than the former, because it is easier of execution, and allows the operator to measure the quantity of fluid transfused. In the present instance I intend employing it. I must, however, observe, that so great is the plasticity of the blood, especially in dogs, that the mere contact of the metal of the syringe suffices to destroy its fluidity, and, in consequence, to render it inapt for circulation. Still, as it is not our present object to investigate the modifications produced in the living hydrodynamic apparatus by the composition of the contained liquid, but simply by its volume, the circumstance adverted to will not seriously interfere with the results of our experiment; so that, instead of introducing the extremity of one vessel by a sort of invagination into the interior of another, I will use an intermediate instrument; I will also modify, to a slight extent, the ordinary mode of proceeding in another way. Instead of venous I will inject arterial blood, taken from an animal of the same species, into the vascular system of this dog. This will, I believe, be the first instance in which the influence of the volume of the circulating liquid on the interior pressure supported by the vessels, will have been directly examined. The contractility of the ventricular fibres ought, in theory, to remain unchanged; to undergo neither augmentation nor decrease of energy, because the blood conveyed to the heart will be neither more debilitating nor more exciting than habitually.

Here are two dogs, of about equal strength, only one is older than the other. The right carotid of both has been laid bare, and the instrument applied to each by M. Poiseuille himself. In order to avoid tiresome circumlocutions, in describing the experiment, we will call the dog from which we abstract the blood No. 1; and the animal into which we inject it No. 2. The jugular vein of the latter has been exposed, and a canula fitted to receive the point of a syringe introduced into its cavity. The columns of mercury stand unmoved, because the cocks have not yet been turned. I now turn them; the mercury in both oscillates pretty accurately between the same points. Thus, we have in

No. 1. 75—100; 75—100; 70—90; 65—95 millimetres.

No. 2. 70—105; 75—105; 65—100; 60—105 millimetres.

It appears, then, there is no very marked difference in the height to which the mercury rises in the two cases. If both animals breathed isochronously, in such manner that the movements of inspiration and respiration should exactly correspond, it is probable that both scales would mark the same degree. I had forgot to tell you that the left carotid of the dog No. 1, has been laid bare, and an opening made in it, for the purpose of receiving the mouth of the syringe. These preparations were made before the lecture, for they would have taken up too much of our time, had they been adjourned till the time of performing the experiment. There is another advantage gained thus,—the animals have had time to recover, to a great degree, from the state of agitation into which they were thrown; besides, the operation is so simple, that it is quite enough to have seen it performed once, in order to understand it fully.

I now aspire some of the blood contained in the carotid of No. 1, by raising the piston of the syringe. The latter fills; and, indeed, to produce this result, there is no necessity for any effort on my part; for such is the energy with which the heart drives the blood forward, that the piston is raised at each contraction of the ventricle. This might even serve as a means of estimating the force of impulsion of the blood, though, of course, the information thus obtained would be only vaguely approximative compared with the mathematical precision of the results obtained with M. Poiseuille's instrument. I, therefore, called your attention to this phenomenon rather as a curious occurrence, than as one susceptible of any important application. The syringe is now filled. The scale marks

No. 1. 60—85; 55—95; 70—80 mill.

I inject the blood into the jugular of the other dog; we have

No. 2. 70—100; 65—90; 70—90 mill.

There is no very marked difference in the height of the two columns. We have, so far, acted with too little liquid to produce any very notable effect. It would even appear that a slight fall, instead of a rise, has taken place in No. 2. I will repeat the injection. The capacity of the syringe I employ may be estimated at about four ounces. The blood had no time for coagulation, for it remained a few seconds only in the instrument, and the latter had been previously warmed to the temperature of the blood. The syringe is now filled a second time. The mercury oscillates between

No. 1. 70—100; 50—65; 60—75; 60—80 millimetres.

The fall is very slight. The column acted on by the heart and blood of the other animal, gives

No. 2. 65—95; 70—100; 75—95; 60—90 millimetres.

The contents of a third syringe are now transfused. The scales indicate

No. 1. 40—70; 60—85; 50—75 mill.

No. 2. 70—100; 75—100; 60—95 mill

There is clearly some diminution of pres-

sure in the dog supplying the blood; but it remains uniform in the receiving animal. I now fill the syringe for the fourth time, and inject its contents into the jugular. We find

No. 1. 60—80; 50—65; 33—55; 45—70; 25—50 mill.

No. 2. 75—90; 70—80; 70—90; 60—95; 70—95 mill.

Consequently, the increase and decrease of the mass of the blood are not proportional to the height of the columns of mercury. The pressure is sensibly diminished in the animal which we have rendered almost anæmic; it is normal, or perhaps slightly lowered, in the dog whose vascular system has been distended by our injections. These results are not literally such as I had announced that we should probably obtain; but we will recur to the experiment, which must be repeated before it can be considered to have furnished precise conclusions incapable of being controverted.

The degree of frequency of the pulse of the two animals is remarkable; that of the dog No. 2, beats only 72 a minute; that of No. 1 beats 150 in the same time. The frequent contractions of the heart, in the latter case, are to be explained by the constant necessity for a new supply of liquid felt by every part of the economy, and by the maintenance of the equilibrium of the currents of blood. The frequency and extreme smallness of the pulse, in cases of abundant loss of blood, have been noted by all observers; they are even very valuable signs of a certain internal hæmorrhage. I shall now let the blood of the animal, from which we have already drawn so much, flow freely. A large opening is practised in the carotid. The mercury stands at

No. 1. 25—30; 25—27; 20—30; 23—25; 20—25 mill.

You perceive that in proportion as the blood flows the pressure diminishes. I need not explain to you why the jet of the liquid grows so feeble; it is perfectly natural that its force of progression should decrease in the direct ratio of the weakened energy with which the muscular fibre contracts. I now tie the vessel, in order to prevent the animal from perishing of hæmorrhage. I must confess that, in spite of the positive information acquired through M. Poiseuille's instrument, I am at a loss what opinion to hold respecting the agency of the blood on the pressure of the walls of the arteries. What a few days past appeared to me decided, seems to me now far from possessing that character. The mercury, it is true, fell to a considerable extent in the dog which we deprived of almost all its blood; but it did so at the close of the experiment only, at a time when the vascular system had retracted, in virtue of its elasticity, and ceased to be dilated by the currents of blood.

Now, in the physiological state, the arteries are not only full but distended; we cannot, therefore, legitimately conclude that the same condition of things exists in that state as in the experiment I have made before you. As the physical conditions are not the same, the phenomena produced must also differ. Although my theory has, perhaps, not been demonstrated to be wholly fallacious, yet I cannot conceal from myself that I took an erroneous view of the influence exercised by the increase of the mass of the blood. First, I injected water into the veins, and the mercury fell. I was surprised at this result, but, in reflecting on it, fancied I might justly ascribe the diminished arterial pressure to the debilitating action of the liquid injected. There might, I well knew, be some other cause, which I was unable to discover, and this was a point, I also knew, on which experiment alone could decide. I consequently made the necessary experiment, and from its result it would appear that, in truth, the volume of the blood has only a trifling influence on the force with which the column moved by the heart presses on the coats of the arteries. I dare not draw a premature inference from a single fact, but we have just seen that the gradual injection of more than a pound of blood did not increase the elevation of the mercury; far from this, towards the close of the experiment it oscillated below its normal level.

These questions, Gentlemen, are not canvassed in our most recent works on physiology; nay, more, they are not so much as mentioned in them. The authors of those books are too fully occupied in discussing the part to be assigned to the *vivifying fluids*, the *materials of reparation*, *assimilable principles*, *&c.*, to spare time for the consideration of hydraulic phenomena. In this matter, therefore, we have no authority to refer to, and must, for the time being, be contented with the evidence of our first essays. But do not suppose that a candidate for graduation would do well in giving proof of knowledge of this kind to his examiners; he would not be understood. No; but let him take the internal maxillary artery, for example, from its origin, follow it through all its flexuosities, name all its branches, and enumerate the anomalies to which it is subject, and he will be sure to receive unanimous praise. If he were asked to describe the functions of the fifth pair, it is true that he would be sorely embarrassed; but he can describe the course of the corda tympani with unerring precision, and institute most learned antitheses between the thick and short, and the long and slender branches. This factitious knowledge is just what is calculated to make a brilliant show at examinations. If it were even obtained by dissecting, something might be said in its favour; but, in the majority of cases, ana-

tomy is studied by plates, as geography by maps; and, provided proof be given of a certain share of memory, the rest is of no consequence. But to return; you see, from the uncertainty we feel respecting the modifications induced in arterial pressure, by the volume of the blood, that all study on the matter has heretofore been neglected. Nevertheless, there is no question more intimately connected with the practice of medicine. Whether you prescribe diluents, or remove blood by venesection, you alter the mass of the living liquid in circulation, and hence produce, inevitably, certain mechanical results. We know that in many cases the pulse becomes less frequent and strong after one or more bleedings; but practitioners are contented with the simple knowledge of the fact, without looking for its explanation. I am of opinion that we shall never obtain really scientific notions on these points, until a totally new view has been taken of them. The modifications ensuing in the frame are the consequences of changes produced in the volume and composition of the liquids; hence the hydronamic phenomena of the body must be studied before the vital.

Before quitting this subject for the present, allow me to bring to your recollection some experiments, elucidating the influence of agencies, of quite a different character, on the pressure of the blood. In those experiments, which were made on the venous trunks, we saw that the arterial pressure is not modified by the physical condition of the tubes, or of the contained fluids alone. We ascertained that in the circulating system, as in the ensemble of systems, constituting the animal economy, vitality claims a share in the regulation of the organic functions. Every moral impression and strong sensation reacts on the heart, and alters the rhthym and energy of its contractions. We all know that anger and fright, for example, betray themselves to the bystander by the sudden change of colour in the features. According as more or less blood flows towards the capillary system the face reddens, or grows pale; it is justly termed the mirror of the passions, because on it are reflected the modifications that occur in the vascular apparatus.

It had already been shown by M. Poiseuille, in the case of frogs, that any considerable pain increased the internal pressure in the vessels; I repeated the experiment, you will recollect, on a dog, in order that you might have ocular proof of the fact. The animal had already served us for some experiments, and, to save it the torture of a new operation, I made my observations on the eighth pair, which was bared by a wound exposing the carotid. The sensibility of the pneumogastric nerve is, you know, far from being exquisite. When I raised it on the director this fact appeared very distinctly, for the dog gave no evidence of feeling the contact of the instrument. Indeed, as I have already had occasion frequently to observe, the sensibility of that nerve is extremely variable. You will not, in all probability, find two species among animals, nay, two animals of the same species, in which the eighth pair possesses a perfectly identical share of sensibility. In some it is enough to touch the nerve to excite cries and convulsive movements; in others you may irritate, and even lacerate it, without the animal appearing to be conscious of what you are doing. It is a still more curious fact, that the sensibility of the two nerves differs in the same animal. You may cut the vagus nerve on one side, and no sign of suffering be manifested; whereas, if you divide its fellow, the section determines all the symptoms of acute pain. I know not on what this difference depends; but, in fine, the nerve under consideration must be esteemed to be endowed with only slight sensibility in the dog. I made the experiment under the idea that the varying level of the mercury would point out more accurately even than the movements of the animal itself, the degree of sensibility of the nerve.

The scale, previous to the experiment, marked from 20 to 22 millimetres; the respiration was regular, and performed with freedom. I compressed the nerve with a pair of forceps; no evidence of much feeling followed; the animal remained quiet. On increasing the pressure considerably, he struggled violently, and the mercury rose to 35, 45, 38, 40, 35 millimetres; but I was at a loss what share in the elevation I should ascribe to the contractile force of the heart, and what to the movements of expiration. It was certainly possible that the ascent of the column was more immediately caused by the play of the respiratory organs than by the increased impulsion of the heart. However, you know that pain borne in silence acts quite as powerfully on the circulation in men, as when accompanied with the most noisy demonstrations; frequently, indeed, stifled suffering deranges the functions of the heart to a greater extent than that which is clamorously expressed, without any attempt being made to struggle against the instinct that causes us to utter cries when suffering physically. Nothing of this kind was, of course, to be apprehended in the case of the animal on which I experimented; he cried out when he felt pain, and ceased to do so when he ceased to suffer. His arteries, indeed, complicated the problem I was trying to solve, and prevented me from forming an accurate estimate of the energy of the ventricular fibre. On repeating the compression of the nerve the column rose, though the animal did not make any considerable struggle; now it stood at 30, 28, 35, 40, 37, 43 millimetres.

The pulsations of the heart became frequent and tumultuous; the stethoscope, applied to the precordial region, transmitted distinct shocks to my ear, which were audible even by the persons surrounding the animal. The respiratory movements became accelerated, so that it would have been impossible to draw any rigorous conclusion from this experiment, had not its results been confirmed by a crowd of previous observations. I finished by dividing the nerve. At the instant the section was performed the animal moved slightly, and the mercury rose a few degrees. All then became calm, except that the respiration, as a matter of course, grew difficult, because the eighth pair presides over the function of the pulmonary apparatus. The surface of the column stood, with little variation, between 20 and 22 millimetres.

I followed up this experiment by inquiring into the influence of galvanism in this way. It is my wish, as far as possible, to perform perfectly new experiments, for by doing so both you and myself are equal gainers. Every one knows the sensation determined by the contact of the electric spark; but no one had thought of studying its effects on the general circulation, though it was plain these might furnish a curious subject for investigation. I drove two platina needles into the animal, one into the cervical region, the other into the middle of the thigh. The two poles of the battery were brought into communication with these needles, so as to produce an electric current through the tissues. The plan generally followed in practice is to apply the conducting wires to the surface of the integuments, and so induce a sudden commotion of the whole frame. In pathological cases I prefer electro-puncturation by means of needles placed in the course of the nerves; I am, in this way, more certain of the action of the electricity, especially as I can limit it at pleasure. In the instance I am speaking of I made use of only ten pairs, lest the battery should be too strongly charged. They were quite sufficient to produce as powerful an effect as was desirable. The moment I touched the needle with the wire, the animal gave a convulsive start, rapid as lightning, and the mercury rose at the same instant to 55 millimetres. The rapidity of the ascent was so great, that it could not possibly be ascribed solely to the efforts of the animal. Something must have acted instantaneously, and directly on the walls of the vessels.

We may conjecture the phenomenon to be explicable as follows:—Muscular action favours the passage of the blood into the veins, as is clearly shown by the acceleration of the flow of blood from an opened vein at the bend of the elbow, by the movement of the muscles of the forearm. Now, if the muscles exercised energetic pressure at the same moment on the entire venous system, the force of progression of the currents of blood would increase to an enormous extent. This is, I fancy, just what happened in my experiment. An electric current traversed the tissues, and the coats of the arteries and veins being pressed on all sides, pressed, in their turn, on the liquid they contained. All authors who have written on tetanus, have described the pulse to be tense, hard, and vibrating. I have no doubt but that these peculiarities depend on the pressure exercised by the blood on the walls of the vessels. To prove this experimentally, we should apply the hæmodynamometer to an animal poisoned with nux vomica.

An equally curious phenomenon, bearing on the present question, was some time past observed by M. Poiseuille, in the course of his researches. A dog had been tightly attached to a table, to serve for some experiments on the pressure of the vessels. The instrument was applied, and the mercurial column stood at rest in its interior. Suddenly the mercury rose several millimetres without the animal's having made any considerable movement, or very deep inspiration. To what, think you, was this sudden ascent owing? To a sudden twinge of pain? No, Gentlemen, far from being caused by a disagreeable, it was produced by an erotic sensation. The fact is, that a bitch in heat was at that moment brought into the room, and her presence awakened in the animal the instinct of copulation. It was a moral impression, and not physical suffering, that, in this instance, modified the circulation of the blood.

But to return to our more immediate study of the properties of the blood in the human subject. There is at present, Gentlemen, in my wards at the Hôtel Dieu, a young female, aged nineteen, whose parents forced her, two years ago, to marry against her will. The consequence has been the development of violent hysterical symptoms. The paroxysms occur frequently in the course of the day, and while they last the patient suffers under frightful delirium, and the strangest hallucinations. She fancies she sees her mother approach to strike her; her countenance then expresses horror and affright; at one moment she appears to try to defend herself, at least some broken words seem to indicate this; at the next she turns suppliant, implores her tormentors to spare her, and, bursting into tears, throws herself into the arms of the persons about her. It would, I think, be impossible to render with more fidelity and expression the different sensations she experiences. The most consummate actress could not equal her in the varying play of the physiognomy, on which one may read in turn despair, dejection, and grief, so truthfully depicted, that one longs for the skill of the painter. This poor pa-

tient fancied that loss of blood would relieve her. Aware of the influence of the moral over the physical being, in affections of this kind, I yielded to her desire, rather to satisfy her, than from any conviction of the efficaciousness of the proposed remedy. Here is the blood extracted; the serum is to the clot as 37 : 26. The proportion of the former is evidently very considerable; and, in addition, the consistence of the clot is but slight. Both agree in showing the existence of a remarkable modification in the liquid. As I have already repeatedly told you, I have no intention of referring all diseases to an alteration of the blood. I am not a partisan of exclusive systems, and, therefore, I will not draw any inference, for the present, from the facts we now observe. I simply note them as appearing very curious; we shall, possibly, be glad to avail ourselves of them at a future period.

I now turn to a point which I consider of some importance, especially as a particular view respecting it has been strenuously urged in favour of the doctrine of inflammation. I allude to what has been termed the *buff* of the blood. It has been asserted (and among others, by Rasori, in a long work on phlogosis) that this is the inflammatory element *par excellence*, and that it is never wanting under certain given conditions, such as during the existence of pleurisy, pregnancy, &c. This notion, advanced perpetually, and defended in several thick volumes, appeared to me to deserve serious examination. We should, above all things, seek for truth; and, therefore, though I see nothing in the *buff* but so much fibrine, which as it is lighter than the colouring matter of the blood, rises to the top and forms into a mass,—I would almost say organises itself on the upper surface of the liquid,—still I set about searching for this appearance in the assigned conditions. With this view I had four or five gravid women bled at different periods of pregnancy; three of them were affected with pleurisy, and yet in not a single instance did this *buff* show itself. Was the inflammatory element afraid to face me? The fact is that I found no such thing, no matter how anxiously I searched for it. However, I should add that the modification in the coagulating process, which produces the buffy coat, depends on a variety of circumstances. Thus, although inflammation evidently exist, if the opening made in the vein be narrow, or if its parallelism with the wound of the integuments be imperfect, and the blood flows slowly; or if the receiving vessel does not present a wide, and, according to others, a narrow, surface to the air, the *buff* refuses to show itself, and remains concealed within the cells of the clot. However, I propose to continue vigorously my inquiry into this matter; for the service done the science by overthrowing fallacious theories, which interfere with its progress, is not inferior to that of making useful discoveries.

Here is another specimen of blood, drawn some days ago, which has caused me considerable embarrassment. It comes from a young Savoyard girl, aged twenty-three, recently arrived in Paris. She was admitted with some symptoms of the fever called entero-mesenteric, or typhoid, such as injection of the conjunctiva, general uneasiness, prostration, colic, and vomiting. Still the blood removed by a first and second venesection seemed of a normal character; at least, you may see that the modification, if any exist, is not appreciable. I therefore felt uncertain about the diagnosis, and, at all events, I think the disease will not prove dangerous. The blood shall also undergo a special examination, and the issue of the affection will show if I have been in error as regards the diagnosis.

You remember, I trust, the case of the female asphyxiated with the fumes of charcoal, whose lung we examined a few days past, and have not forgot the fluidity of her blood. I purposely return to her case, because a singular phenomenon was ascertained respecting it after our separation. I never before met with a similar peculiarity, not even in cholera patients. Her blood is notably acid; you perceive that it reddens litmus paper. I cannot be sure that this character proceeds from the entry of carbonic acid gas into the circulation, but venture on this explanation of its existence, as a plausible hypothesis, worthy of further examination. The exact analysis of the liquid might, perhaps, give us some positive information on the point. We will try, at all events.

In pursuing our study of the blood we must constantly keep in view one fundamental fact, namely, that the blood in the living animal is quite a different fluid from that extracted from the body. Medical observers and chemists have, in general, paid too little attention to this difference. By studying the liquid in the laboratory alone, they have increased the perplexity of many points, and in their statements almost all of them are at variance with each other. In truth, there is a vast difference between the fibrine while circulating with the blood, and when separated from that fluid. Besides, the blood presents itself in two very distinct conditions, even when removed from the body. In some cases it forms a compact mass, its superior surface being of a bright-red colour, the lower part blackish; in other instances it separates into two perfectly distinct parts,—one solid, called the clot; the other liquid, known as the serum. Although our means of investigation cannot be made applicable to the blood, except when removed from the body, nevertheless we will improve on those who have preceded us, by not confounding it with the fluid that traverses our organs.

It is from this view of the subject, and on just grounds, that very meritorious observers, among others Professor Müller, of Berlin, have distinguished the *liquor sanguinis* that circulates in the vessels, from the liquid which separates from the blood when extracted from the body. Thus, when we examine such vessels of an animal under the microscope as are sufficiently transparent for the purpose, we distinctly perceive an infinite number of globules, borne along by a rapid movement, rolling and slipping over each other; and we see, besides, between the mass in motion, and the walls of the vessel, a space almost destitute of globules, and filled with a colourless transparent fluid. This fluid is the *liquor sanguinis*, which holds the globules in suspension during life. It is probable that, by examining the blood while performing its various offices, we should succeed in discovering some of the phenomena of its organic composition. Unfortunately such study is rendered almost impossible from the absence of favouring circumstances; we are reduced to very limited observations on small animals, in which some of the organs are transparent enough to allow the eye to follow the different movements, and catch the form of the globules under the microscope. Very young rats and mice, and bats, are almost the only mammiferous animals on which these experiments may be made with any success. At the present moment the rigour of the season has benumbed the latter animals, and I have not been able to procure any. It is impossible to conceive why the phenomena of the intimate composition of the blood are enveloped in such complete mystery: it would seem as though Nature took pleasure in hiding their essence from us, precisely because it is a matter of such paramount importance to comprehend it. The *liquor sanguinis* is, no doubt, in reality serum, but serum which holds in suspension, or in solution, the coagulable matter or fibrine of the blood. Fibrine, by the way, is an improper term, and ought to be replaced by *coaguline*; this is, possibly, not unobjectionable, but it would, at all events, prevent us from confounding under the same denomination two different substances, the fibrine of the blood, and the fibrine of muscle. Chemists fall into a palpable error in regarding these two principles as identical; and the proof that they are not so, in addition to the absence of resemblance in their physical properties, is, that compared in respect of their alimentary qualities, they differ widely. I have demonstrated, by direct experiment, that the fibrine of blood is but slightly nutritive, whereas that of muscle is extremely so. The serum, again, is the *liquor sanguinis* deprived of its fibrine; the latter becomes organised, differing herein from the albumen of the blood, which always forms into an amorphous mass under the action of heat and the acids. When removed from the vessels part of the serum solidifies, the rest remains liquid. Such is not the constitution of the circulating fluid. The study of the clot presents similar difficulties. In the living blood, as it might be called, there is a liquid holding particles in suspension; but if you receive some of this blood into a vase, a clot will form and the globules disappear, while within the vascular tubes they move in the midst of a viscid liquid, endowed with properties with which you are acquainted. In a vase solidification takes place: one of the elements of the liquid, the fibrine, becomes organised, and imprisons the globules in its cellular structure. In the living subject the serum contains the globules and fibrine in suspension; removed from the body that liquid bathes a clot composed of fibrine and globules.

Here, then, are points of material difference. Still, of the two parts, the serum and the clot, into which the blood separates when out of its natural tubes, examine the latter, and you will find that the liquid dissolves the globules. The fibrine will alone remain, and you will be astonished at its minute quantity, compared with the dimensions of the clot, and the enormous number of globules it contained. The fibrine, which might be termed the basis of the clot, is, in this isolated form, worthy of the most serious attention. It, alone, is at once the cause and the agent of the solidification of the blood. The latter is one of those phenomena placed on the confines of vitality and physical endowment, and one which might, in a certain sort, be compared to the symmetrical transformations undergone by matter called crystallisation. However, these two kinds of organisation must not be confounded; if you examine one of these masses of parenchymatous fibrine under the microscope, you will detect a regular conformation in it resembling the forms assumed by organised matter, such as ramifications and areolæ, intermingling and anastomosing in an infinity of ways. The clot, then, the *insula* of the ancients, must not be looked on as inert mass, but as a fibrinous arborescent matter, forming the basis of a finely and delicately organised parenchyma, and differing essentially from the albumen, the solidification of which is the simple result of a chemical or physical agency. But the functions of the fibrine do not stop here. It is to be found again, with the same characters, in the coagulum that obliterates divided arteries or veins: it is to be traced in the formation of adhesions, of false membranes, and of cicatrices; and it is to be seen deposited in layers at the surface of solidified wounds. Under all these conditions it is organised; its arborisations are converted into pseudo-membranes, and these false membranes become canaliculated, forming vessels permeable, in their turn, by the

liquid that produced them, and of which they originally helped to constitute the substance.

Much has been written on the cicatrisation of tissues divided by the cutting instrument. The growth and adhesion of granulations, the effusion of coagulable lymph, and the organisation of new vessels, have all of them given rise to important researches. One thing remained to be done, namely, to examine the influence of the composition of the blood on the rapid or slow, complete or incomplete, formation of cicatrices. Now, the animals I had defibrinised were well adapted for experiments on this point. I chose a dog for the purpose, from whom I had successively removed several portions of fibrine (this, by the way, is an experiment to which I shall call your attention at a future period); he was greatly reduced in strength; still he was not so much pulled down as to be unable to survive an operation. I made a longitudinal incision through the skin and some depth of muscle in the anterior and middle part of the neck. The blood that escaped from the divided vessels appeared more liquid than usual; it did not coagulate on the blade of the bistoury. I united the lips of the wound accurately by the twisted suture, and the animal was left to himself. He died yesterday evening, having survived the operation a few days only. Here is his body, which I now proceed to examine. We may, before beginning, indulge in a few conjectures, as we shall be enabled to verify them immediately. The main question is, did union by the first intention take place? Opinions might be divided on this point. For my part, I believe that such union was impossible, because the blood contained no fibrine, and had, therefore, lost its power of solidification. I express my opinion thus distinctly and unreservedly, because, even if it be erroneous, it cannot leave any wrong impression on your minds, as the autopsy will at once correct it. I now shave away the hairs surrounding the wound. The blood, which had by its desiccation glued its lips together, masks their appearance, and prevents us from seeing if they have really united. However, if I may judge from what I can already perceive, there is no real adhesion. The tissues are discoloured, dry, and hardly at all swollen. In the situation of the points of suture, there are, no doubt, a few cellular bands; but these are rather isolated false membranes, than an exact adhesion of the two surfaces of the solution of continuity. The cicatrisation of the wound, therefore, could not be accomplished; and everything seems to show that, if the fibrine had been completely withdrawn from the blood, there would not have been any points even of apparent union. You will often see wounds of bad character in the hospitals, which remain stationary for weeks or months, resisting all the efforts made to hasten their progress to a cure. We may dress them with stimulants, cover them with powdered bark, sprinkle them with the chlorides, but all in vain; no improvement follows. The case alters when, as is sometimes done, leeches are applied in the neighbourhood; the effects are then soon perceptible,—but what are they? The rapid extension of the ulcerated surface both in length and depth! But let the patient be placed in favourable hygienic condition; let a strengthening diet replace that to which he had been confined, and his strength returns; his face regains its colour; the wound assumes a vermillion hue, and advances quickly to cicatrisation. The consequence deducible from these facts is too evident to require me to expatiate on it. Do you not see that the therapeutical means employed were useless, so long as they were calculated to act on the solids alone, but that they at once became efficacious when the liquids had been modified? The explanation of this is simple. As the solids draw from the blood the materials proper for their cure, they continued in a diseased state, so long as that liquid was itself altered, and could not furnish those materials. From the moment its normal composition was restored to the blood by a suitable regimen, there was nothing to impede cicatrisation.

I made another somewhat similar experiment on this animal. I tied a large vessel with the intention of examining it after a certain time to see if a coagulum was formed within it. It is not very uncommon to observe recurrence of hæmorrhage after the fall of a ligature; and it seems possible that that serious accident might be prevented, if we had more accurate notions on the coagulability of the blood. I consequently applied a ligature to the right carotid; let us see if it is obliterated in such a manner as to be capable of resisting the heart's impulse. I cut away a piece of the artery, in order to slit up its walls, and examine its interior with greater ease. You see that there is no trace of coagulum; for I cannot give that name to a few granules which lie on, without adhering to, the internal membrane of the vessel. The ligature had already nearly cut through its walls; had the animal survived a few days longer secondary hæmorrhage must have taken place.

This rapid view of the subject will allow you to form a notion of the importance of the study of this organisable material, which reconstitutes, reproduces by itself alone, all the tissues of the economy. We will push our researches respecting it as far as possible, and shall probably obtain results, both of a physiological and therapeutical kind, more precise in their character than those we now possess.

I have now to inform you of the present condition of some animals submitted to va-

rious experimental treatments. All these experiments refer to questions of the deepest interest, such as, among the rest, the relative proportion of the serum and clot. In the blood of a healthy and robust male the serosity may be estimated as forming a fifth or a fourth of the whole; it abounds with fibrine and globules. In women and children the serosity constitutes one-third of the mass. But these proportions vary according to the age, temperament, and species of nourishment of the individual, as well as a variety of other circumstances. My object is to determine what proportion between the two elements is incompatible with the continuance of life. This is an experimental problem completely. In the hope of solving it, I have placed different animals in circumstances fitted to give useful indications respecting it. We know that successive bleedings, closely following each other, augment the proportion of serosity, and induce various disorders, and finally death. I have submitted one animal to this species of treatment. The quantity of serum increases also, from the abundant use of drink; at least, this is a notion generally admitted by medical men. But once again, we are reminded that we must not prejudge results; our prophecies are constantly exposed to be proved to be false. Here, for example, is an experiment well calculated to make us cautious in this respect. I have had two ounces of blood taken from the animal before me every day, and replaced by an equal quantity of distilled water. I fancied I should have had to present you a specimen of blood rich in serum, and containing a small proportion of clot. The reverse is, however, the truth; it contains, to all appearance, scarcely any serum. I presume, however, that the serum is really augmented in quantity, but that some cause or other prevents us from appreciating its increase. Perhaps it is retained in the substance of the clot. A third animal is also bled daily; he is given plenty to eat, but no drink. At the second bleeding the serum was found increased in quantity, and it had, besides, lost its limpid appearance. A fourth animal has abundance of drink, but no solid food; his blood contains as much serum as clot. Here is the product of the seventh venesection practised on the animal about which I have already more than once spoken. Although he eats and drinks as much as he likes, his health is materially affected; a notable change has taken place in his gait, habitudes, and temper. The mucous membranes have grown singularly pale,—a peculiarity which has been long noted by veterinary practitioners in appreciating morbid symptoms. When he is bled now, syncope follows. I have no doubt but that an affection of the lungs will soon come on, and speedily put an end to his existence.

Finally, here is the subject of an experiment of a different kind. I injected some frog's blood, the globules of which are ovoid and provided with a central nucleus, into the veins of this little animal. I am anxious to see whether they will become transformed, so as to acquire the characters of the globules of mammiferous animals. The subject of the essay is, as you perceive, in good health, and does not appear in any way affected by this new species of transfusion.

UNIVERSITY COLLEGE HOSPITAL.

ABSTRACT OF LECTURES ON THE EXCISION OF JOINTS; BY MR. LISTON.

Delivered October, 1838.

Lecture I.

This was the first clinical lecture which Mr. Liston gave this session. He said he had few opportunities of meeting the students as a lecturer; there was, indeed, little encouragement for him to do so, except that feeling of pleasure and satisfaction which every one must experience in the imparting of knowledge to others. No kind of instruction was equal to clinical instruction, in which the pupil might see practised that which was described to him. But for the full benefit of clinical teaching, it was necessary that the cases should be recent and fresh in the minds of the students; old cases raked up, after a long period, were comparatively useless. The subject he had selected for the present lecture was that of diseases of joints, a variety of which cases had lately been in the hospital. The disease of the joints requiring resection or amputation, might occur in a variety of ways, in all or any of the tissues entering into the formation of the articulation; or it might be the result of injury to the soft parts in the neighbourhood; thus injury of the knuckle, or a bruise, might be followed by abscesses forming along the member, and the articulation become diseased. The same fact held good with respect to the larger joints; the disease, however, in these instances, generally commenced in some part of the apparatus of the articulation itself. The signs and symptoms of disease, as affecting the different structures, were, at the commencement, generally sufficiently distinct; but after a time the whole of the structures became involved, and the signs, of course, altered. The disease most frequently commenced in the synovial membrane of the articulation, and was of a chronic character. In this case there was often so much swelling, that the form and exact situation of the joint was, with difficulty, ascertained. Sometimes, particularly in the knee and

elbow, fluctuation was detected. In this state the synovial membrane was thickened, more vascular than natural, its surface soft and pulpy, of a brownish colour, and the secretion became increased and vitiated. The swelling was also caused, in many instances, partly by effusion, external to the joint itself. If this state of matters was not promptly attended to, the disease would go on until the articulating surfaces of the joint became absorbed, and the ends of the bones affected by an obstinate and unmanageable caries. The disease would, after a time, take on a more acute action, and the joint, instead of being filled by bad synovia, would contain pus, and the synovial pouches come to resemble exactly the cysts of abscesses in the soft parts. The disease was generally slow in its progress, and the pain was not violent until the cartilage became absorbed, and the bone exposed. Ulceration might, however, originate primarily in the cartilage, and then there would be severe pain from the commencement; but this ulceration might exist for a considerable period, and even extend to the osseous tissue, before any great swelling of the joint came on. The disease was supposed sometimes to exist when there was only a derangement of the general health. Complaint would be made of violent pain in the part, but it was found in such cases that, when the patient's attention was directed another way, the freely moving the ends of the bones forming the joint did not aggravate the symptoms, and there was no violent pain in these cases at night. In cases where the joint was actually diseased the pain was violent and constant, and much increased by motion. Ulceration took place at various points of the articulating surface, when around the margins the eroded parts were filled up by fringed processes of the synovial membrane, in the same way that the worm-eaten looking parts of bone, in process of separation in necrosis, were filled by vascular membrane, springing from the external or internal periosteum. There was no doubt, however, that the cartilage was vascular, and could be removed or repaired, independently of any assistance from the neighbouring tissues. This he had maintained long since in lectures, long before they were published, and also in his surgical works. This opinion he was glad to find maintained by such an authority as Sir Benjamin Brodie. They had before them specimens showing ulceration in parts of the cartilage which were opposed to each other, and removed far from any portion of normal or diseased synovial membrane. Various drawings were also shown to illustrate the same point. Cartilage was also absorbed from that surface adhering to the bone, and was in consequence often separated in scales. Ulceration of cartilage, occurring as a primary affection, was generally accompanied by purulent secretion; but, in many instances, it was observed to have occurred, to a considerable extent, in limbs that had been disused, as from bad fracture, or in phthisical or bedridden patients. Sometimes, again, there was much purulent matter in the joints without any ulceration, as in instances in which pus was separated from the blood, or absorbed; this occurred in a case lately under the care of Mr. Cooper, and which was not very properly marked "phlebitis." (See LANCET, vol. i., 1838-39, p. 210.) If the pus, however, continued long, then the cartilages would become absorbed and the bone exposed. A case illustrative of this had occurred in a patient who had met with a very severe accident on the railway. In this case pus was effused into the shoulder and other joints; the cartilage of the head of the humerus, as they would observe, had become absorbed, and the bone extensively exposed. When the cartilage was primarily affected the other tissues soon became involved, and when the synovial membrane began to suffer, all the signs were present that are observed when that membrane is first affected. Sometimes, again, the disease commenced in the head of the bone itself, of which he would show instances. The bones, as they might see, contained tubercular matter; it was not uncommon to find this kind of deposit in the heads of bones of young people. Here was an instance in which there was softened tubercular matter in the head of a bone. In this case an abscess had formed in the epiphysis, and burst into the joint, leading to the necessity for amputation.

In the treatment of those affections of joints, where the disease was confined to the synovial membrane, and the patient experienced only slight uneasiness, with unsteady motion of the limb, gradual and uniform support of the joint was employed with advantage by the methodical application of plaster. A woman in No. 6 ward, as they must have seen, had a large globular swelling in the knee, which was brought nearly to its normal size by giving it this support; it had in a few weeks been reduced several inches in circumference. Then the limb could be steadied, and supported for a time, by the application of splints fitted to the joint. The splints might be made, according to circumstances, of lint soaked in mucilage, or of the skirt leather. More active practice in this kind of cases was seldom required. When the cartilages were affected, whether primarily or secondarily, then it became necessary to be more energetic in the treatment; added to the absolute and perfect rest of the limb, counter-irritants were often required, one of the most effective kinds of which was the formation of an issue on one or both sides of the articulation. It was necessary, however, to be

cautious in making an issue too near an articulation, as in the knee-joint, where there was not much covering. He had seen a case in which an issue was formed over the condyle of the femur for diseased knee, in which the issue actually passed into the joint, and amputation of the limb was required rather from the treatment which had been pursued, than from the disease. The principal means of cure, then, in all cases was the *complete rest* of the affected part. It was astonishing to observe how the general health of patients improved after they had been a short time in the hospital, when this repose of the part was given. It could well be imagined what, in the advanced stage of disease, great irritation would be produced by the grating and rubbing of the ends of the diseased bones against one another. The constitutional treatment must not be neglected; tonic medicines were advisable; and in children the preparations of iron were, perhaps, most beneficial. Sometimes, however, in spite of the greatest care, the disease made progress; sometimes, again, the treatment was not understood; while, in other instances, the patients neglected to apply for relief until the means above-mentioned would have no beneficial effect. The relations of young patients sometimes fancied that dislocation of the joint existed, and went from one practitioner to another, regular and irregular, until after much pulling about, in some cases, the articulation became almost irreparably damaged. Bone-setters, ignorant of the matter, might be excused in their anxiety to make work for themselves, if they now and then cracked their fingers, and made people suppose that a joint, simply sprained, had been displaced, and by their superiority and cunning in manipulation, reduced with a snap. Professional men sometimes, however, fell into mistakes on this point which were productive of much immediate suffering, and subsequent misery and mischief to the patient. He remembered the case of a boy who slightly sprained the hip-joint in leaping a trifling grip, but walked two or three miles the day after. Here dislocation was supposed to exist, and means taken to reduce it, and on one of the occasions the powerful attempts were continued for a period of two hours. The little boy laboured under morbus coxarius. In many cases, then, the patient, from neglect or otherwise, had abscess form in or near the articulation, the synovial membrane and ligaments became disorganised, and the abscess burst through the soft parts; hectic comes on, and if proper treatment be not employed the patient sinks. Even in advanced stages the disease, however, in some cases, was remedied without an operation, provided the patient was sufficiently vigorous. Anchylosis, in these cases, occurred, and the patient had a stiff-joint. In cases of long-continued disease, where the health of the patients was bad, palliative means were of no avail; it became necessary, then, to take steps to relieve the system of the source of irritation, and it frequently became necessary to amputate the limb for the purpose of effecting this, and thus saving life. As Mr. Pott had remarked, "a determination in such cases for or against amputation, is really a determination for or against the patient's existence." Sometimes, however, the limb might be saved by having recourse to a piece of "clever butchery," "to use a phrase," said Mr. Liston, "much employed by a scribbler in one of the quarterlies. Such is the way in which efforts, painful enough to the feelings of any humane and conscientious surgeon, to save life, are talked of. This snivelling humanity-monger seems to be annoyed that those whom he lends his support to, are not so much trusted as they would wish in difficult cases—in those requiring operative procedure. It is the old story of the fox and grapes; they have not many cases demanding the dexterous use of the knife, and they, therefore, make a practice of decrying operations, and those who can and do perform them properly. When they happen to be so employed, their proceedings may be designated as "butchery," if they choose; they do not always deserve to have the adjective "clever" applied. It probably never entered into the calculation of these persons that an individual who has qualified himself to operate with rapidity and dexterity, with as little pain and effusion of blood as possible, may also have studied disease in all its various shapes; may be as able to form as good a diagnosis or prognosis, and take means to check diseases in its earlier stages, so as to avoid the necessity of resorting to the use of the knife, fully as well as if he had studied at the most fashionable hospital in town. The "clever butchery," then, alluded to, consisted in an operation which was originally proposed by Mr. Park, of Liverpool, about fifty years ago, in a letter to the celebrated surgeon of his day, Mr. Pott. The operation was neglected for a long time, but was afterwards practised by the Moreaus, and subsequently by Mr. Crampton, Mr. Syme, M. Roux, Mr. Key, himself, and others, and was now getting popular; it consisted in the excision of the affected parts. Excision, however, was only applicable in some joints, as in the shoulder, for instance; he, Mr. Liston, had performed it twice on the ankle-joint. It had been practised on the knee, also, but the result was not very encouraging. It has been frequently practised on the elbow-joint with the best success.

ESSAYS IN SURGERY.

By J. M. Coley, *Esq.*,
Senior Surgeon to the Bridgnorth and South Salop Infirmary.

No. III.

DISLOCATION OF THE RADIUS FORWARDS.

Before the publication of Sir A. P. Cooper's great work on dislocations and fractures, the displacement of the upper extremity of the radius over the front of the external condyle of the humerus, was so seldom discovered, that it was considered impossible by some of the most eminent surgeons, notwithstanding a case had been communicated by the French Academy of Surgery. Desault, one of the surgeons to the Hôtel Dieu, disbelieved that it could ever occur, and Mr. Samuel Cooper, the compiler of British and foreign surgery, also doubted whether such an accident could ever happen:—"Hence," observes the latter, "accidental dislocations of the radius, suddenly produced by an external cause, must, if they ever happen, be exceedingly uncommon at its upper end."†

For a description of the accident, and an account of the appearances found on dissection, the reader is referred to Sir A. P. Cooper's work before alluded to, p. 482.

The two first cases, which came under the notice of this celebrated surgeon, after every attempt then known to effect replacement of the dislocation, were left unreduced; but his experience in two subsequent cases led him to adopt a proceeding, which proved successful. The mode of reduction recommended by him consists in extending the radius alone by applying the extending force to the hand, previously rendered supine. The following case illustrates the advantage of combining with this mode of extension the assistance of the radius as a lever, which, I feel assured, is the most effectual and speedy mode of accomplishing our wishes.

Case.—Aug. 9, 1833. Master Jenkin, of Alscotte, ætat. 10, was thrown from a horse. The elbow came, with great force, in contact with a stone on the road, which fractured the internal condyle of the humerus, and forced the upper head of the radius forwards over the outer condyle. The forearm was semi-flexed and the hand prone. The radius admitted of rotation, and its dislocated head could be seen in motion at the same time in its new situation; the forearm could neither be bent nor rendered straight beyond a certain point. There was a puckering of the integuments just above the dislocated head of the radius, and the fractured condyle was felt projecting below; so that, at first sight, the displacement appeared to be lateral.

The accident having occurred two days before I saw the patient, the parts adjoining the injury were swollen and inflamed. Extension, according to Sir Astley Cooper's direction, was tried without success. I then placed the arm over the back of a chair with the external condyle uppermost, and confining it in this situation by an assistant, I bent the forearm downwards, gently extending the hand at the same time, and thus readily replaced the head of the radius.

† Dictionary of Practical Surgery. Second edition, p. 324.

EFFECTS OF RESPIRING CARBONIC ACID.

To the Editor of The Lancet.

Sir:—Having endeavoured to call the attention of the Medical Section of the "British Association for the Advancement of Science," to the subject of "the Comparative effects of Human Respiration, and of the Combustion of Charcoal upon the Atmosphere," &c., by the accompanying essay (which was intended to have been read, with some slight modifications, at the meeting of the Association at Newcastle, but was interdicted, by reason of its having been printed, although it was stated to have been printed solely for the purpose of gratuitous distribution amongst such of the members as might think it worth their time to peruse it at their leisure), I send you some of the results of an experiment made by myself relating to the subject in question.

On the night of the 24th inst., at twelve o'clock, I retired to my library, a room 16 feet 6 inches long, 13 feet 10 inches wide, and 9 feet 4 inches high; its capacity was 2129.82 cubic feet. The chimney was closely built up with bricks and mortar. The window was very large, but was perfectly tight as to the admission of air. In this window were two casements, each 3 feet 1 inch high, and 1 foot 2 inches wide; each of them fitted well. The door was 6 feet 5 inches high, and 2 feet 10 inches wide; it fitted well against the stopping strips, but it left a vacancy of about a twelfth of an inch from the floor; and having a key-hole of the ordinary size, it may be said to have afforded a space for the ingress or egress of air, equal to an aperture 3 inches long and 1 inch wide. The temperature of the room was 54 deg. Fah.

The stove employed on this occasion was on Harper and Joyce's principle, and was about 7 inches in diameter and 16 inches deep. The inverted cone, through which the air entered at the bottom of the stove, was perforated with 12 holes, each about ¼ inch in diameter. The ventilator on the top was about 4 inches in diameter, and of the "wheel" construction; it was left perfectly open.

The charcoal was of the common kind, and not recently prepared; the stove could contain 4lbs. avoirdupoise of this charcoal, which always required about two hours combustion before the aqueous vapour was entirely dissipated. On several occasions I had weighed the stove, with its contents, immediately after the complete dissipation of its aqueous vapour, and again after an interval of from 3 to 4 hours. The loss usually averaged after the rate of 1 ounce per 18 minutes. When fully charged with common charcoal, 4lbs. of fuel would always maintain combustion during from 20 to 21 hours, the ventilation being quite open all the time. This stove was charged with fuel, and lighted at 11 P.M. on the night of the 24th inst.; it was placed out of doors for one hour, and then taken into my room, and placed at a distance of 7 feet from the front of the sofa upon which I lay, and at right angles to my head. The sofa was 3 feet wide and 2 feet high, consequently my head may be supposed to have been about 8 feet 6 inches distant from the stove, and perhaps about 2 feet 3 inches above the floor. Having fastened the door inside, I lay down in a thick, woollen dressing-gown at 12. At 4, A.M. I felt a slight degree of giddiness, which was scarcely perceptible, unless any attempt was made to turn upon the pillow. This increased until 5¼ A.M., when my sensations resembled, in every respect, those felt by many persons when at sea, viz., intense vertigo, aggravated by the slightest motion; a desire to be sick, without the power; great prostration of strength, and apparent inanition, or want of capability to move any muscle of the body. To these were added a very full, throbbing, quick pulse, producing an impression upon the brain as though the arteries were rapidly distended to their very utmost capacity; the maximum impulse being accompanied by a peculiar thrill, resembling that produced by a light touch upon a piece of catgut, which had been strained until it was about to break. The cephalalgia was of unwonted and agonising violence, and particularly affected the occipital regions. I felt no symptoms of suffocation, although I could easily have fancied myself poisoned. It was evidently time to move, so I quickly slid over the edge of the sofa, and tried hastily to open the nearest window. My strength failed; I fell immediately upon the cushion of the sofa, and in less than a minute was literally streaming with perspiration. In a few minutes I made another effort to open the other window, and with great difficulty succeeded. I crawled upon my hands and knees to the sofa again, and for a short time remained insensible. The fresh air, however, soon revived me, and I recovered sufficiently to get to the door, and opened it, and dragged the stove into the adjoining passage, and got again to the sofa, where I remained for 13 hours, enduring the utmost distress. About 7 A.M. my wife came into the room; she saw me very ill, but I could not give her any information as to the cause. She invited me to take some pulv. rhei., which was the only medicine she could command. I nodded assent, and it was with the greatest difficulty that I could sustain myself in any position to swallow it.

In the course of the day (about 2 P.M.) a medical friend, Mr. T. Davis, of Nailsea, accidentally called upon me. He saw me in my predicament, and prepared a few effervescing draughts, which soon set the rhubarb in action. At 7 P.M. I dressed myself, and while everything was fresh in my memory I tried to write; but the headach returning with the effort, I was obliged to discontinue. At 12 last night I retired to the same sofa, all things remaining as during the preceding night, with the exception of the stove, slept well, and this morning am almost recovered.

If one ounce of charcoal was consumed in 18 minutes, 18.33 ounces would be consumed in five hours and a half. If 12.94 grains of carbon be equivalent to 100 cubic inches of carbonic acid gas (Dr. Thomson), 18.33 ounces, or 8019.375 grs., of carbon will be equivalent to 61973 cubic inches, or 35.85 cubic feet, of carbonic acid gas. If the respiration of an adult human being consume the oxygen of 151.1 cubic feet of atmospheric air (vide Essay, p. 13) in 24 hours, it will produce 30.22 cubic feet of carbonic acid in that time, or 6.92 cubic feet in five hours and a half. Hence the total quantity of carbonic acid gas, eliminated by both stove and respiration, amounted to $35.85 + 6.92 = 42.77$ cubic feet.

Now, the total capacity of the room was 2129.82 cubic feet; and, if we could suppose the chamber to have been perfectly air-tight, the total quantity of carbonic acid that could possibly have existed in its atmosphere at the expiration of five hours and a half, would have been but 2 per cent.

	Cub. feet.
The nitrogen would remain as before	1703.86
The oxygen would have been reduced to	383.19
And the carbonic acid, instead of being about 1 cub. foot, would be increased by	42.77
	2129.82

I regretted, exceedingly, that I could not avail myself of a mercurial trough which was standing upon a table between myself and the stove, all in readiness, with receivers filled with mercury, &c., to have retained some of the air of the apartment before the window was opened; but it was absolutely impossible for me to have done so at the proper time.

And now comes the point:—Is carbonic acid a poisonous gas (Dr. Christison)?—or does it kill by suffocation (Dr. Thomson)? I conceive Dr. Thomson's (?) opinion to be correct. In my own case, all the symptoms of poisoning were apparent, and none of those of suffocation, and I attribute them entirely to the noxious effluvia which escaped with the carbonic acid gas; for unless the carbonic acid did really gravitate, which is not at all probable, the quantity proportioned to the atmosphere of the apartment was not sufficient, even at its maximum, *to have produced suffocation*; and I am certain that I have breathed a larger proportion of pure carbonic acid gas, *without being poisoned*.

In the instance of death related by Dr. Christison, of a boy who had been teazed by a party of smiths, who held a recently-extinguished candle under his nose, the effect never could have resulted from the carbonic acid gas, but must have been occasioned by some such noxious principle as so completely enervated yours, most truly,

CHARLES T. COATHUPE.

Wraxall, near Bristol, Oct. 26, 1838.

THERAPEUTIC ACTION OF THE ERGOT OF RYE.

To the Editor of THE LANCET.

SIR:—I trust that the few remarks I am about to make upon the action of the secale cornutum, will not be unacceptable to the generality of your readers. In the cases which I published in the 24th and 1st numbers for Sept. 16th and Oct. 7th, 1837, of the "Lon. Med. and Surg. Journ.," I attempted to show the specific properties of this powerful remedy, and my more extended experience, within the last few months, still further confirms my opinion of its utility in promoting uterine action. It has been stated by many practitioners of eminence, that the use of the ergot not only produces most alarming consequences to the mother, but likewise, in some instances, proves fatal to the child, and that it occasions retention of the placenta. Such effects I have not witnessed, as I always endeavour to prevent the sudden expulsion of the body of the fœtus resulting from the full action of the ergot, and thus to allow the uterus to contract gradually on the secundines, which I invariably find lying within the vagina.

Such being the grounds upon which the opinions respecting the inutility of the ergot are based, I am led to observe, in differing from these opinions, that the drug, when productive of unpleasant effects, must have been either deteriorated in quality or administered to persons of a leucophlegmatic temperament. The only symptoms which I have noticed when given to such individuals have been a sensation of sinking at the pit of the stomach, together with irregularity in the heart's action. In such cases, I generally administer it in a small quantity of warm ale or spirit, which prevents such an occurrence. Ingleby, in his "Treatise on Uterine Hæmorrhage," remarks that, "To denounce the ergot, and deny its efficacy, without an experimental acquaintance with its action, is not only most unphilosophical, but altogether inconsistent with patient research;" and I feel confident that if the spirit of the above quotation had been properly observed, the number of failures would have been comparatively few, since, in my own practice, the action of the ergot has been as manifest as that of opium or mercury.

My experience leads me to consider the following appearances as indicative of the genuineness of the ergot:—If the powder be put into a small vessel (a cup for instance), and about ℥j. of boiling water poured thereon, then immediately covered, and allowed to stand for a few seconds, on removing the cover, the ergot should remain totally insoluble, and the infusion should assume a deep pink colour; on the contrary, if small portions of the ergot be seen floating on the surface of the water, and the infusion present a milky appearance, then the action of the remedy cannot be relied upon, for I have never known it, when given under such circumstances, to produce the least increase of uterine contraction, but invariably to be followed by more or less of a feeling of sinking at the pit of the stomach, attended with irregularity of the heart's action, and total cessation of pain. The above fact has not, as far as I am aware, been noticed by any writer, either on materia medica or midwifery. Should the test I have named, as connected with the administration of the ergot, prove to hold true in other hands, as it has done in mine, I shall be fully rewarded for my labour in endeavouring to find out some way of judging of its purity prior to its administration. Dr. Waller appears to think that if the pains are feeble, though regular, the ergot may produce increased action of the uterus; but that if the pains have ceased, its action is less to be depended upon, and it cannot be trusted when it is found necessary to produce premature labour. Dr. Ramsbotham, in one of his lectures, published in the "Med. Gazette," has enumerated several cases wherein he produced premature labour by means of the ergot alone, and the same result has occurred, in more than one instance, in my own practice.

CASE 1.—I was called upon at 11, A.M., Sept. 26th, 1838, to attend Mrs. R., a woman of stout and robust habit, whilst in labour of her fourth child. I ascertained, on my arrival, from her own account, that she had been in labour for more than two weeks

On examination, I found the os uteri elongated, yet thin, resembling very much the teat of the cow; it readily admitted the entrance of the finger, when the protrusion of the membranes, for a short distance, during each pain, was perceptible. Under such circumstances, I ordered an anodyne draught to be given immediately.

27. I was again summoned this morning, about 9, A.M.; the pains were now considerably increased, but, on examination, I found the os uteri in much the same state; the pulse being full and strong I bled her to ℥vij., and this procured temporary cessation from pain; I then ordered the following draught to be taken directly:—*Tincture of opium*, ℥j.; *peppermint water*, ℥j.; and requested that I might be sent for on the return of the pain.

28. This day, about 6, A.M., I was again summoned, and found my patient suffering from the most acute pain; her bowels had been freely opened, and the pulse was slow and weak. On introducing my finger, per vaginam, I could perceive no alteration in the condition of the os uteri. Having tried all the means that have been recommended, or which I could myself suggest, with the view of inducing regular action, and finding them of no avail, I at once determined upon the administration of the ergot, and accordingly I gave Mrs. R. ℈j. of it in ℥ss. of tepid water, and in ten minutes after its use the pains assumed a more true uterine character, and, on examination, I found the membranes protruding beyond the elongated os uteri, which was also much reduced in length; the labour then proceeded gradually, and in the course of half an hour from the first employment of the ergot, she was delivered of a living son. I may here remark, that the mother and child are both doing well.

Case 2.—On the 29th Sept., 1837, I was desired to attend Mrs. T., 40 years of age, and mother of several children. Her constitutional powers were much weakened from repeated abortions. On my arrival I found that she had miscarried about three weeks before of a four months' fœtus, and the offensive smell on entering the room was sufficient at once to show that the secundines had not come away. She was exceedingly weak from loss of blood; pulse 140, weak and thready; bowels regular; tongue coated with a brown fur; urine passed in small quantities, and high-coloured; lower extremities œdematous; thirst urgent; anorexia, &c. &c. On examination I could easily introduce the finger within the os uteri, and I found the placenta plugging up the mouth of the organ, and the placenta could be easily broken down by the finger. Seeing that no time was to be lost I gave her ℥j. of secale cornutum in ℥j. of tepid water, and in an hour afterwards the placental mass was expelled in a complete state of decomposition. Immediate relief followed its expulsion. I need not detail each day's progress towards recovery, as it was rapid, for my patient was able to sit up and attend to her domestic duties in one month from this time.

The insertion of the foregoing remarks and cases in your valuable periodical will oblige, Sir, yours obediently,

Thos. H. Wardleworth, Surgeon.

Rochdale, Oct. 31, 1838.

PHENOMENA CONNECTED WITH SEA-SICKNESS.

To the Editor of The Lancet.

Sir:—I have read in The Lancet reports of the two discussions which have taken place at the London Medical Society, on the subject of sea-sickness, and as the phenomena and physiology have not been much entered into, may I take the liberty of forwarding you a few lines upon the subject, which is one of great interest. I remain, Sir, your very obedient servant

Robert Stevens.

Kennington Common, Oct. 29, 1838.

In sea-sickness an individual perceives, principally by the incapacity of the muscles which maintain position, a confused knowledge of his relative position with fixed objects; but how this can produce such an affection can only be elucidated by examining certain facts connected with the affection itself, together with analogous affections, and our physical constitution.

Under whatever circumstances a person's knowledge of rest and position becomes confused, the sensation of sea-sickness, the identical affection, is produced. When travelling on a railroad, by observing the quick changes of perspective on the immediate side of the railroad, the affection can be taken up, *ad libitum*, by the eye; whilst during intoxication, in bed, where the loss of position is only imaginary, it can be kept off by the eye being directed to any fixed object. On a rough sea, the loss of knowledge of position, with regard to fixed objects, exists palpably, and the affection lasts till some clue to it can be obtained; and this clue is, in some cases, the perception of perpendicular gravity, with the art of using the muscles to maintain position in the uncustomary motion. In a carriage, the perception of rest and position becomes confused more readily with the back to the horses, hence the affection takes place more readily so. All persons have not equally the innate geometrical faculty of observing and becoming accustomed to changes of position. Some are so devoid of it as to lose the points of the compass after taking one turning in a carriage; hence the degree of liability. The sailor who can keep the account of position unconfused, on board

ship, cannot always do so in a boat, where the motion is more irregular, and he being less accustomed to it. Jactitation is not the only cause of the affection we call sea-sickness. As I have shown above, the identical effect can be induced by any cause that can confuse the perception of rest and position. That jactitation can produce a corresponding motion in our circulating fluids, so as to implicate the brain physically, is at variance with our physical constitution, both as regards the form of the arterial distribution and the laws of hydrostatics.

In sea-sickness the organs become affected in the same order as in syncope produced by mental circumstances; the difference in effect is owing to the exciting causes being unequal in degree and duration. That unusual motion, and other causes by which the perception of rest and position becomes confused, should produce an effect upon the great sympathetic system of nerves, which supplies functional power to the circulating and assimilating system, is no more wonderful than that syncope is induced, in some, by seeing a surgical operation; or that a thrilling about the solar plexus is experienced on entering the examining hall of the College of Surgeons. These are metaphysical phenomena connected with our physical constitution, and that is all we know about them.

Physiology of Sea-sickness.—In sea-sickness the first impression is on the great sympathetic, so that functional power is denied to the assimilating system and to the *circulating system partly.* (See LANCET, vol. 2, 1836-7. p. 692. The causes why *arterial* blood flows from veins in phlebotomy during partial syncope.) Thus, functional power being withheld from the stomach, the diaphragm and abdominal muscles (which are not supplied by the sympathetic, but by the cerebro-spinal nerves) are called into action, and produce vomiting, which tends to excite the heart again; but if functional power were denied in toto to the heart, even for a second, the effect would be not sickness, but syncope and asphyxia.

The best *treatment* of sea-sickness would be to place the patient, with the eyes covered, in a hammock, slung with long strings, as near the centre of the ship as possible, and if any palliative be given, it should be large doses of ammonia with opium.

ARSENIC AS A PRESERVATIVE OF DEAD BODIES.

To the Editor of THE LANCET.

SIR:—As the last Number of your Journal contains an article on preserving bodies for dissection, by injecting them with a solution of arsenious acid, you will oblige me by giving publicity to my reasons for believing such a practice to be highly dangerous, as calculated to expose the dissector to breathe an atmosphere contaminated with arseniuretted hydrogen, which is, perhaps, the most deadly combination of arsenic. Nearly two years ago I first prepared, at the school in Great Windmill-street, a saturated solution of arsenite of potash, for the injection of a subject, at the suggestion of Dr. Hunter Lane, the lecturer on chemistry there, who had read an account of its effects in a foreign journal. The injection succeeded, producing precisely the effects and appearances described in your quotation; by passing into the veins and diffusing itself everywhere from the permeability of dead tissues, the solution, in a short time, left the arteries empty, so as to admit the usual red injection. Two or three more bodies were subsequently injected, and during the dissection of one of them, one of the pupils suffered severely from pain in the stomach and bowels, vomiting, and purging.

In the summer of 1837, I injected another body, and dissected it, with five of my fellow students, during the very hot weather of, I think, August. Decomposition was retarded considerably, but there was only one of us who did not suffer more or less indisposition, principally bowel complaints: and the subject gave out a peculiar odour, which I suspected arose from the arsenic rising in combination with the volatile products of decomposition. I took some portions of the body, at the end of five or six weeks, and subjected them to a careful examination, and though they had been drenched with the solution, in the first instance, I found no arsenic remaining.

As an *experimentum crucis*, I some time afterwards placed some animal substances, in a state of decomposition, on a dish, along with solution of arsenite of potash, and also powdered arsenious acid, and placed over them a bell-glass receiver to collect the gases given off, and, at the end of two or three weeks, I added the air contained in the glass to a sufficient quantity of pure hydrogen to make an inflammable mixture, and burnt this as it proceeded from a small jet, holding a piece of glass in the flame, and I procured a small quantity of metallic arsenic. I expressed my conviction that this mode of injection was dangerous, and it was discontinued at that school. I remain, Sir, your obedient servant,

JOHN SNOW, M.R.C.S.

58, Frith-street, Soho, Nov. 5th, 1838.

Professor Goodeve expressly mentions, in the article, an abstract of which we have given, that no ill effects were experienced by any of the persons engaged in the dissection of bodies injected with a solution of white arsenic.—ED. L.

THE LANCET.

London, Saturday, November 10, 1838.

A FORTNIGHT ago we inserted a note from a correspondent, relating some circumstances connected with the death of Mr. GEORGE FREDERICK LOCKLEY. If we have hitherto made no observations upon the case, and upon the conduct of Sir HENRY HALFORD, it is not from any disregard for the honour, or the morality of the medical profession. The statement seemed so strange, so improbable, so revolting, that we expected an immediate contradiction. The party principally implicated has, at length, attempted to explain this melancholy transaction in a letter which he has addressed to a political journal.

The facts communicated by our correspondent, and re-echoed in the newspapers, were briefly these:—Mr. LOCKLEY, a practitioner in Half-moon-street, London, was invited by Sir HENRY HALFORD to accompany him into the country. Mr. LOCKLEY was on intimate terms with the Baronet, and was exceedingly serviceable to him in a variety of ways. He was now upon the London and Birmingham Railway, on the road to Sir HENRY HALFORD's seat in Leicestershire, where several *distinguished* persons in the county had been invited to dinner. Before the train arrived at Tring, Mr. LOCKLEY fell back on his seat in an apoplexy. The President of the College of Physicians had him conveyed to the station-house, and told the persons there that the individual was highly respectable,—gave Mr. LOCKLEY's address—desired that a medical man might be sent for, and then took his seat again on the train. No medical assistance could be procured—Mr. LOCKLEY was left a prey to this dreadful disease for six hours—till, at last, a physician arrived from London. The seizure proved fatal. The physician of GEORGE IV. arrived punctually at his seat in Leicestershire—his guests were not kept a minute waiting—his cook was not disgraced—and the host did the honours of the table, to the great satisfaction of the gentry of the county; in the meantime Mr. LOCKLEY was abandoned to strangers,—the blood was gushing upon his brain,—in an obscure room at Tring.

The public papers called loudly upon Sir HENRY HALFORD for an explanation of his conduct. The public will, perhaps, be satisfied with the explanation which he has been pleased to offer: the medical profession cannot: where a charge of this magnitude has been deliberately brought against the nominal head of the profession, nothing but a *public inquiry* can be satisfactory. Anything he may say himself is open to suspicion, and is necessarily an *ex-parte* statement. The stain can only be wiped away by the "Not Guilty" of a jury of his peers.

The necessity for a public inquiry is not at all lessened by Sir HENRY HALFORD's letter. None of the features of the case are changed in the slightest degree. Mr. LOCKLEY fell down, he says, in Euston-square, but assured him that it was an accident.

"When we had passed Watford, Mr. Lockley became sick, and soon afterwards fell asleep, breathed oppressively, and did not answer my questions. In this state, when we arrived at Tring I took him out of the carriage, and carried him into the parlour of the inspector of that station, *a most humane person*, who sent off instantly, at my desire, to Mr. Dewsbury, of Tring."

The President of the College of Physicians was perfectly aware that it was an attack of apoplexy: the symptoms were decisive, and left no doubt upon the subject. But Mr. DEWSBURY, or his assistant, visited him within an hour; while our correspondent was led to believe that no medical assistance was procured until six hours after the ill-starred patient had been left to his fate. This, however, was a pure accident. Whether it was five minutes or five hours before assistance arrived, the character of the action remains unaltered. The patient was labouring under apoplexy; not a moment was to be lost; yet the physician left him to take his chance, and to *run the risk of dying* before remedial means were ob-

tained. On this point let us hear his own words:—"Having given precise directions "what should be done, I felt that I could "not be of any further use at present, and "*therefore pursued my journey.*" Oh, touching benevolence! Exalted friendship! Your directions cost you a few words, or did you write them down before you held yourself acquitted of all further concern? Of the propriety of bleeding in Mr. LOCKLEY's case, little doubt can be entertained: Sir HENRY HALFORD *directed* him to be let blood—so soon as medical assistance should arrive. The President of the College of Physicians would not stay to bleed the patient himself, although it may have been the means of restoring him *alive*—not dead—"to the bosom of his family." "If I could have ad- "ministered, instead of directing only, the "expedients of our art to my friend, I "should have done so." Here the President insinuates that he could not stoop to perform venesection—he could only nod—and direct that expedient of our art when his *friend's* life was at stake. Yet it is very well known that the Baronet did not disdain to "administer" injections,—also expedients of our art,—to a princess every morning, for many years. To be sure that princess was an illustrious personage, and Mr. LOCKLEY was only Sir HENRY HALFORD's friend.

Sir HENRY never returned to inquire after the fate of his *friend*. "I should have been "ready to return the next day, but after Mr. "DEWSBURY's first visit, Mr. L. had the "daily assistance of *friends from London*." It was a happy circumstance that the *friends from London* had not the same hearts as the friend in Leicestershire.

Let us weigh the action impartially. A non-professional person invites his friend into the country; if that friend were attacked with apoplexy in the railway train, would he carry him out of the carriage, and leave him to his fate, at a station-house? or would it be his duty to remain with him, and to see that he had medical assistance, and every attention which his circumstances required? The person we have supposed is a physician, and the President of the College of Physicians; he abandons a patient, a friend, a brother, we had almost said—at least, a labourer in a common profession,——insensible,—"breathing oppressively,"—gasping in the grasp of apoplexy.—What is to be thought of such conduct? Would one person in 10,000 have "pursued his journey" with Sir HENRY HALFORD? We feel persuaded that at least 9999 would have remained behind with the unfortunate LOCKLEY.

If neither friendship nor kindness could have swayed this physician to a discharge of his duty, a regard for consistency should have had that effect. Upon all occasions—in orations, and in scientific conversazioni,—he has obtruded his religious creed and feelings upon the profession and the public. He has done this in the most offensive manner. He has not merely declared himself a pious, Christian, moral physician, but he has said to every Licentiate of the College, upon a thousand occasions, "I am better than thou!" And he has ascribed his superiority—with a Pharisaic humility—to the moral discipline of the English Universities. He admitted, before the Parliamentary Committee, that the physicians educated at other Universities were equally well versed in their profession, but would not place them on the same footing as those from Oxford and Cambridge, *because* they had "undergone no moral and intellectual "trial in the Universities from which they "came." Would a Licentiate of the lowest moral feeling have abandoned Mr. LOCKLEY?

We shall not stop to inquire how this action accords with the precepts of Christianity, of which the President of the College of Physicians has said so much in his recent homilies. But we must compare it with a parallel case, put by the Founder of the Christian Religion, after he had directed a lawyer to love his neighbour as himself. A man going down to Jericho fell among thieves, who wounded him, and left him half dead. A priest passed by on the other side; a Levite looked at him, and passed by

on the other side; a certain Samaritan, who made no pretensions to orthodoxy, had compassion on him, *went to him*, and *bound up his wounds, pouring in oil and wine*, and (as the traveller was not going by railway) set him on his own beast, and brought him to an inn, and *took care of him*. Now, it appears to us, that the Pharisaic priest, who passed by on the other side, treated this stranger very much as the President of the College of Physicians treated his friend, when he "pursued his journey;" and that the conduct of the Samaritan, who dismounted, braved the dangers of the road, and bound up the sufferer's wounds, presents a remarkable contrast to that of the physician, who left his friend by the wayside, at issue with apoplexy, without attempting to administer to his relief. The President would have incurred no danger by his humanity, and the trouble would have been inconsiderable. But the Samaritan had no *distinguished* visitors in Jericho, expecting him to entertain them at dinner. No. Certainly not; *that* alters the complexion of the case.

The President, in his lame defence, tells the public that "his conscience does not accuse him." A great many victims of public prosecution are in the same predicament; their consciences accuse them of nothing; and, in their own esteem, they are very meritorious characters. Presumptuous Pharisee! Remorse has never crossed your brow—the ghost of LOCKLEY has never broken your slumbers—but dare not again, in the presence of your brethren, to raise your eyes to Heaven with an ostentatious piety, or the powers which smote your abandoned friend may dart their lightnings upon your head, and for other reasons.

Was LOCKLEY's seat empty at the banquet? Did the President's cold eye never start from that?

It remains to be seen whether the College of Physicians will sanction, and in some measure adopt, the conduct of their President in this case, by passing it over in silence; or whether they will vindicate the honour of the profession in the eyes of the public, by instituting a searching inquiry. What course will the renegade Licentiates adopt? What will the men do who deserted the ranks of CLARK, and ARNOTT, and HALL, and so many excellent Licentiates—who blackened their public characters for ever—and apostatised from liberal principles, that they may enjoy the *honour* of being *Fellows* of one who abandoned a friend to play the APICIUS of a county?

WE take the following statement from the "Staffordshire Examiner" of Oct. 20th. It occurs under the head of "Finance," in the Report of the Goal Committee of the county, which was read by Mr. HARTSHORNE, the Chairman, at the General Quarter Sessions, during the transaction of the county business:—

"The increased expense of coroners' inquests arises from alterations of the law, on which your Committee are not called upon to comment. They cannot, however, avoid calling the attention of the Court to the following facts:—During the last quarter Mr. Smith has held 82 inquests, and has called in the professional assistance of a surgeon only *in three cases*. Mr. Cattlow has held 10 inquests, and only called in a surgeon in *two* cases. While Mr. Harding has held 25 inquests, and has in nine cases called in a surgeon. Mr. Seckerson, out of 10 inquests, has called for that assistance in no less than seven cases. Your Committee are aware that it must rest with the coroner to determine the propriety of calling in surgical evidence; at the same time due regard should be paid to the expense entailed upon the county from requiring such evidence, and due caution against incurring that expense without adequate reason. It appears from the present schedule of allowances for expenses of inquests, that 1*s.* is allowed to each juryman for his attendance; considering the very trifling nature of this remuneration to individuals, while it entails an expense of between £300 and £400 per annum on the county, your Committee venture to suggest that it should in future be discontinued."

It might be supposed from this, and other paragraphs of a similar description, that the increase of expense to the county has resulted from the operation of the Medical Witnesses' Act; but the reverse is the fact. That measure has secured compete

medical testimony, when it is required by the Coroner and Jury, and medical practitioners are not dragged miles from their homes without receiving compensation for their labour,—a result which is utterly at variance with the harsh and unjust treatment to which they had been subjected during centuries. The increased expenses connected with Coroners' Courts have been produced, not by the statute which we have mentioned, but by the Act of 1837, which gave to Coroners an increase of one-third of their previous fee upon every inquest; and this additional remuneration was one of the consequences of the *savings* which have been created by the Poor-Law Amendment Act. By relieving the poor-rate of some very trifling and insignificant payments, charges, to the extent of many hundreds, have been thrown upon the county-rate. Wise legislation!

A Treatise on Inflammation. By JAMES MACARTNEY, M.D. Longman and Co. London, 1838.

(Concluded from page 241.)

AT the close of our notice, in last week's LANCET, of Dr. Macartney's "Treatise," we had arrived at the chapter in which the consequences of inflammation are examined. These are distinguished into real and reputed. We have already mentioned the former; the latter are, according to Dr. Macartney's views, effusion of coagulable lymph and ulceration. Hitherto the effusion of lymph has been looked upon as one of the means of reparation consequent upon inflammation; and hence the inflammatory process, to a certain degree, has been encouraged for the purpose of providing the medium through which wounds are supposed to become united. The author of the present Treatise, however, distinctly shows that the effusion of coagulable lymph, although frequently coexisting with inflammation, is not a necessary consequence of it, and that reparation may take place without any of the phenomena of inflammation being present. The sanative process thus goes on, while the evil element with which it is so often conjoined is eliminated. Upon this important point Dr. Macartney remarks:—

"Coagulable lymph may be thrown out by a natural and healthy action, as in the formation of the decidua uteri; that it is eminently conservative in arresting hæmorrhage from opened vessels; in the union of all the soft parts when divided; in forming the medium of conjunction of fractured bones, and in constructing the walls of an abscess and of an aneurysmal sac. Immediately on the receipt of an injury, also, lymph is shed before there is time for inflammation to set in. The surface of a wound that does not bleed is covered by a layer of lymph in the very moment that the injury is inflicted. The inflammation which would ensue from the opening of a serous cavity is sometimes altogether averted, and almost always restrained within certain bounds by the effusion of lymph, uniting the opposed surfaces with each other."

The question, then, between Dr. Macartney and his opponents, may be briefly expressed in the following terms:—Injuries are repaired by the effusion of coagulable lymph, the consequence of inflammation; therefore, *excite* inflammation, and you effect a cure. Dr. Macartney says, coagulable lymph is shed *before* the occurrence of inflammation, which, so far from being a sanative, is an *injurious*, process; *prevent*, therefore, the development of inflammation, and you obtain an union which resembles, as closely as possible, the natural process of reproduction. The two theories are as opposed as any two theories can well be, and the records of surgery prove how deeply the practice of the healing art has been influenced by one of them.

John Hunter, in his work on inflammation, described three modes of healing, viz., union by the *first intention*; union by *adhesive inflammation*; and union by means of *granulations*; but, as his commentator, Mr. Palmer, informs us that, at the present day, the two first species are considered to form but one and the same, we may reduce the number to two. Dr. Macartney, on the contrary, informs us, that reunion and reorganisation are effected in *four* different ways:—

First, by immediate union, without any intervening substance whatever. We have a familiar example of this mode of healing, in slight cuts received on the fingers, which sometimes heal in a few hours, without the individual having any unpleasant sensation in the part after the moment of the infliction of the wound. There are few of our readers who have not, at one period or another, experienced a slight accident of this kind,

and we appeal to them whether, under the circumstances described, they have ever witnessed the phenomena of "pain, heat, tumefaction, and redness." There is not a peasant, we will venture to affirm, sufficiently ignorant or unobservant not to know that the chances of quickly healing a cut finger diminish exactly in proportion as the phenomena just mentioned manifest themselves; yet, according to the theory of the schools, he should hail the appearance of these phenomena with delight, for they are the signs of inflammation, and inflammation is a *sanatory* process. It is vain to say that in the case just described there exists a certain quantity of inflammation, just sufficient to heal the wound nicely and comfortably, and no more. This sophism is disposed of by Dr. Macartney in the following passage:—

"If there be any degree of inflammation in which there are no heat, redness, tumour, pain, or disturbed vascular action, it ought to be clearly distinguished from that kind which is attended with these phenomena; and then we should have two sorts of inflammation, the one with phenomena, the other, without; which, if we chose to disregard the logical contradiction involved in such an admission, would amount to the same practical result as if on one occasion inflammation did exist, and on others it did not."

The second mode of union is by the medium of coagulable lymph, or a clot of blood; the third species of reparation consists in reorganisation without any medium of lymph or granulations, the cavity of the wound becoming obliterated by a natural process of growth. This mode, which the author names the "modelling process," has never been described, because surgeons, heretofore, did not know that open wounds in man, as well as in the lower classes of animals, could be healed without inflammation. The pain of the injury, in cases of this kind, soon ceases; there is no tumefaction, and the edges of the wound are so strongly disposed to approach each other that they cannot be kept asunder by mechanical restraint; the surfaces of the wound, although they come into contact, do not unite by vessels shooting across; they are smooth, red, and moistened by a fluid, which is, probably, serum; the wound gradually contracts, and is finally healed by the same means which determine the shape of the natural parts of the body. In some cases the wound is cicatrised before the surfaces are abolished, after which the natural growth goes on, until no part of the original wound is left; the succeeding cicatrix is small, pliant, and free from callosities; no trace of inflammation can be discovered during the process; and the author has known patients to be so entirely free from all uneasy sensations as to be ignorant of the real site and extent of the injury until they had examined the part with their hand, or saw it in a looking-glass. The fourth species of reparation is that which takes place through the medium of granulations; but we must pass over the observations of Dr. Macartney upon this point, as also his remarks upon cicatrisation and the reparation of different tissues. The description of the mode of reparation in necrosis is peculiar to Dr. Macartney, and differs in some essential points from modern theories; but the conclusions at which he has arrived are drawn after a careful investigation of this disputed subject, and are, moreover, sustained by an extensive series of injected preparations, now deposited in the anatomical museum of the University of Cambridge.

Having examined, in successive chapters, the local and constitutional causes of inflammation, Dr. Macartney proceeds to consider its proximate cause, which he regards as an organic sense in the injured part. The following passage contains the essence of the author's doctrine upon this disputed point:—

"When an injury has been committed on any part of the body, the natural sensibility of that part is roused to a consciousness of what has been suffered, either instantaneously or in a very short time. This local feeling or impression, or whatever it may be called, is distinct from the sensible pain of a wound, and may exist in the greatest degree where little, or, perhaps, no pain is felt; at other times it may be coincident with common pain, although, when perceived, it is distinguishable from it. In general the organic consciousness is distinct from that of the individual, unless the sense of injury be communicated to the whole nervous system, and then the person feels a degree of agitation and alarm, which he cannot understand or account for. On some occasions we observe the immediate effect of the impression shown by the vessels of the part, as when the circular blush around the puncture, at the moment of vaccination, gives intimation of the sensibility of the part to the admission of an animal poison; and sometimes the individual is conscious of a peculiarity of sensation from the contact of other

morbid poisons, when the organisation has not been injured, but has merely received the impression.

"As we have sufficient grounds for assuming that an organic sense of injury, or danger, modified according to the causes which produce it, necessarily precedes inflammation, it is reasonable to conclude that this is the *proximate* or *essential cause* of inflammation, and the opinion seems to be confirmed by the history already given of the constitutional and local causes, as also by the contemplation of the kind of sensibility and vital properties which belong to the tonic tissues, of which the arterial is one."

The preceding reflections embrace a theory which we believe to have originated with Dr. Macartney, although it has been adopted, with numerous modifications, by several modern writers on medicine. We cannot dwell on it, for we hasten to arrive at the practical portion of the work, which occupies nearly one-half of the entire volume. The means for mitigating or abolishing inflammation are classed, by Dr. Macartney, under seven heads, viz.:—

1. Remedies which diminish the force of the heart, and dispose the small arteries to contract.

2. Means which reduce the sensibility of the inflamed part and the volume of the arteries.

3. Medicines which augment the natural secretions.

4. Counter-irritants.

5. Sedative and astringent lotions.

6. Applications which are agreeable to the sensibility of the affected parts.

7. Causes which produce an easy or satisfied state of feeling on the sentient surfaces, or in the individual.

It seems to us that this number might be reduced by throwing together one or more classes; but this is rather a technical than a practical objection.

Dr. Macartney passes under review each of the above-mentioned means, and points out their mode of action, interspersing his observations with practical remarks of the greatest interest. Thus, we would particularly recommend to the reader's notice the remarks on the therapeutical effects of bloodletting, and the manner in which this (generally considered *simple*) operation should be performed. The modes of action of heat and cold are also ably investigated, and practical rules are laid down for the employment of those powerful agents. With regard to the action of counter-irritants, the author offers a very ingenious physiological explanation. He observes that they do not act by deriving or transferring the original disease, but may, perhaps, produce their beneficial effects on the principle of exciting an artificial disease, which, being always disposed to cease or recover, creates the same tendency in the original inflammation. This, however, is merely put forward as a conjecture. The application of cold should be regulated by the feelings of the patient. Whenever cold lotions alleviate pain they do good, and whenever they have not this effect they are improper. But the application of water, either under the form of steam, or kept in constant contact with the affected part, by means of what Dr. Macartney calls "water-dressing," is one of the most powerful means which we possess for the treatment of inflammation.

We have already pointed out the peculiar part of Dr. Macartney's *theory* of inflammation; the *practical* portion, which may be said to belong almost exclusively to him, is the treatment of inflammation by steam and water. We shall remark, that the author does not claim to have DISCOVERED the utility of water as a topical antiphlogistic; on the contrary, he traces the history of this remedy from the time of Hippocrates to Paré, Percy, and Larrey, of the French school, to Professor Kern, of Vienna, and to a few modern surgeons who have followed the example of their predecessors. Dr. Macartney's remarks upon this point leave no room for future cavil or reclamation.

"It is quite manifest (p. 188), from the history I have given of the employment of water in its liquid state, for the cure of wounds and inflammation, from the earliest periods to the present time, that I do not claim the discovery of the remedy; but that I have been the means of introducing it to the profession in these countries is a matter of too much notoriety to admit of dispute. I have also connected the use of it with general views of inflammation, which, whether right or wrong, are peculiarly and distinctly my own; and I have demonstrated the possibility of open wounds healing without inflammation and without the medium of either coagulable lymph or granulations, a fact which, as far as my information extends, has not even been hinted at by any writer on surgery, either ancient or modern."

It were to be wished that some of our *discoverers* of the present day would imitate the modesty here displayed by the veteran Professor of the University of Dublin.

The length to which our analysis of Dr. Macartney's work has extended compels us to refer our readers to the "Treatise" itself for a description of the "steam apparatus" invented by the author, and of the mode in which he uses "the water-dressing," as well as the effects produced by these powerful agents. The perusal will well repay the trouble, and enable them to direct a means of treatment which unquestionably is superior to any other hitherto proposed for the cure of simple local injuries. In conclusion, we do not hesitate to pronounce the "Treatise" of Dr. Macartney to be the most original medical work which has appeared since the days of John Hunter,—a worthy companion to the "Teoria della Flogosi" of Giovanni Rasori.

Medical Reform; being the subject of the first Annual Oration instituted by the British Medical Association, and delivered by A. B. Granville, M.D., &c. London: Sherwood and Co.

We strongly recommend to the notice of our readers, both professional and non-professional, the "Oration" of Dr. Granville on the subject of Medical Reform. Dr. Granville has taken for his text-book the Report on Medical Education, which was printed by order of the House of Commons, on the 13th of August, 1834. From the rich fund of information contained in that important document, and from his own investigations, Dr. Granville has collected a number of facts which demonstrate, in the clearest manner, the necessity of medical reform, and the steps which should be pursued for the attainment of this vital object. The moderate price of the pamphlet, the important topics discussed in it, and the pleasing, though forcible style in which Dr. Granville has clothed his arguments, will, we feel convinced, ensure an extensive circulation to the discourse upon medical reform.

WESTMINSTER MEDICAL SOCIETY

Saturday, October 27, 1838.

Mr. F. H. Thomson, President.

OBSCURE DISEASE OF THE CERVICAL VERTEBRÆ; COMMUNICATING WITH THE ŒSOPHAGUS.—PURPURA.—SIR A. CARLISLE'S MISTAKE.

After the ballot had closed,—

Mr. J. G. Smith related the following case:—A young lady, twenty-four years of age, who had been "ailing" for some time, while on a visit at a friend's house, was advised to consult the family surgeon; this she did, and that gentleman thought, from the symptoms present, that it was a case of common dyspepsia, and ordered medicines accordingly. On coming back from the consultation she felt unusually fatigued, and went to bed, where she experienced a slight rigor, which was soon followed by pyrexia. The family surgeon, on visiting her, saw nothing to cause alarm, and merely sent her some febrifuge medicine. On the following morning she complained of a numbness in the right leg; in the course of the day, the other leg became similarly affected. She was bled; but the next morning the patient was worse, and Mr. Smith was requested to see her. The symptoms now, on the afternoon of the second day, consisted in a paralysed state of the lower extremities, both as regarded sensation and motion. She passed no water for 48 hours, a symptom she had before disguised, and which had no doubt produced much suffering. There was fever; the tongue was coated; she had had rigors both the evening before and next day. As no evidence of any mischief going on in the head could be detected, the spine was thought to be at fault; it was examined with particular care, and slight irregularity was fancied to exist at the junction of the last dorsal with the first lumbar vertebra. Considering, then, that some affection of the spinal marrow existed, she was cupped and blistered over the affected region, and calomel and opium administered internally, without the production of any benefit. For two or three days she went on much in the same way; the urine being drawn off with a catheter; it was offensive and high-coloured. The febrile symptoms became slightly mitigated, but she was still paralysed; the urine was still retained; and the fæces passed involuntarily. At the expiration of a week, she recovered slight sensation of the right leg, but none in the left, which was subject to spasmodic twitches which much distressed her. It was ascertained, after very minute inquiry, that some months before she fell from a chair as she was endeavouring to reach some article from a high shelf, and that she wrenched her back, which troubled her for a day or two, but had not since af-

fected her in any way. She had, however, for the last three or four months, apparently been in a cachectic state, but complained of nothing but pain about the epigastrium, and the vomiting of "bilious matter" in the morning. During the time the lower extremities were paralysed, tickling the surface produced contraction of them and a pain in the back. Things went on from bad to worse, the urine became ropy, fœtid and alkaline, and contained a quantity of mucus; she had a severe rigor, followed by anaccession of fever, and she sunk suddenly.

Post-mortem Appearances.

On laying open the cavity of the chest, the lungs presented a healthy appearance, excepting a firm adhesion on the right side between the pleuræ at the upper part, with considerable thickening, the effect of organised lymph, which formed the boundaries of an abscess communicating with the following morbid condition of the spine:—The surfaces of the bodies of the first dorsal and the last cervical vertebra were found denuded of their periosteum, and bathed with pus of a deep yellow colour; extending upwards, behind the œsophagus, was a sinuous channel, about two inches in length, which communicated with the œsophagus by a free fistulous opening, and had obviously been the course by which the abscess had for a length of time discharged itself. On examining the bone more minutely, it was found that the intervertebral substance between the last cervical and the first dorsal vertebra, was entirely removed, and the two bones firmly anchylosed, both by the junction of their bodies, and also by their oblique processes, without having produced, however, any particular distortion or projection which diminished in any way the size of the spinal canal, and apparently consisting of a simple subsidence of the last cervical vertebra. The anterior surface of the dura mater was encrusted with a firm portion of organised lymph about the size of half-a-crown, corresponding to the anchylosis of the vertebra. The sheath being opened, the medulla spinalis was perfectly healthy, and its functions had been probably interrupted by the pressure exercised by the effused lymph.

The viscera of the abdomen were healthy. The urinary bladder alone presented any morbid appearances; the mucous membrane was highly vascular and somewhat thickened, presenting small ulcerated spots, with shreds of slough undergoing a process of reparation; a small quantity of urine, mixed with blood, alkaline, very offensive, and loaded with ropy mucous. The kidneys rather vascular.

The case related was interesting, chiefly in consequence of the obscurity in which the pathological condition of the spine was involved. There was nothing to lead the practitioner to the seat of the disease. For five or six months before it had been noticed that the respiration was carried on entirely by the diaphragm, though latterly she had been observed to expand the chest. The arms towards the last became slightly numbed. It had been since ascertained that one of the family had died from abscess of the hip-joint, and another of them from diseased spine.

Mr. H. Thomson said, that in cases where abscesses formed in the spinal column, and the matter was prevented from escaping, sudden death might result from the pressure produced by the fluid. He had lately examined a little girl, in whom the matter from a spinal abscess had been very freely discharged; it was checked from some cause or other, and she died suddenly while drinking her tea. A considerable collection of pus was found in the vertebral canal, in the neighbourhood of the fourth cervical vertebra.

Mr. Verrall thought that the case, as related by Mr. Smith, was particularly instructive, as affording evidence of the progress of a formidable disease of the spine, without any one suspecting its existence. He believed disease of the spine existed more frequently than was suspected by many practitioners. How did Mr. Smith account for the suddenness of the death in his case? Simple pressure alone would not have been sufficient to produce it. He had seen cases in which persons were entirely paralysed by pressure or by division of the spinal chord, and yet they lived a long time after. He had lately seen a man in Essex, who some time since fell from a tree, and had since been entirely paralysed, having had neither motion or sensation. His health was good. He only could move his head, and with his mouth wrote and drew exceedingly well.

Dr. Addison considered that if spine disease was occasionally overlooked, it was suspected to exist in hundreds of cases in which it was entirely absent. The latter error was a far more prevalent one. He remembered a case in Guy's Hospital something similar to that related by Mr. Smith, in which a patient was admitted for what appeared to be croup, and treated accordingly, but suddenly died. In this case the mischief was situated in the cervical vertebræ, though no evidence of such disease existed during life. The trachea was normal. In the case related by Mr. Smith, it was no wonder that much doubt existed regarding the origin of the symptoms. Paralysis of motion and sensation was often observed in connection with the nervous and uterine affections of young females, and which gave way on the restoration of the general health. Nothing more was observable at first in the present case. But there was one symptom latterly, besides the rigors

which indicated much mischief, and one, indeed, which he (Dr. A.) had never known to exist, except as an indication of lesion of the spinal chord. He alluded to the spasmodic twitching of the legs, mentioned, too, by Mr. Smith. However fallacious other symptoms of spinal disease might be, the one in question he had never known to deceive.

Mr. STREETER said, that injuries of the back, however apparently slight, should be carefully watched. Slight injuries would occasionally produce chronic inflammation, and lead to results, which, if not always so serious as observed in the present case, were, nevertheless, to be dreaded. In ricks of the back high up, when the body was weak, chronic inflammation might go on without pain. There was great necessity of attending to such accidents, especially where it was a twist.

PURPURA.

Dr. ADDISON inquired if members had observed purpura to be more than usually prevalent; he had lately seen some most severe cases of this affection, both in hospital and private practice. Had any member any suggestion to offer regarding the treatment of this obscure disease?

Mr. HAY had found opium and acetate of lead of very great benefit in a very bad case he had care of two or three years since.

Dr. REID had found the ergot of rye, in ten-grain doses, three times a day, of very great service in passive hæmorrhages.

SIR A. CARLISLE'S MISTAKE.

Mr. CHINNOCK, at the close of the meeting, said, that having seen the President's name in one of the journals, in connection with a case of delirium tremens, he trusted that gentleman would favour the Society with a paper on the above disease at the next meeting, in order that the subject might be discussed by the members.

The PRESIDENT said, he should never shrink from the performance of what he considered a public duty; but he thought if it was suggested that a paper should be read by him, he should be at liberty to choose his subject. He, therefore, proposed to read a paper at the next meeting on irritable breast.

[We understand it is the intention of Mr. Chinnock to bring before the Society the subject of delirium tremens, in reference chiefly to the diagnostic marks between that disease and insanity.—REP. L.]

Saturday, November 3rd, 1838.

Dr. CHOWNE, President.

SINGULAR MALFORMATION.—IRRITABLE BREAST AND ITS TREATMENT.

MR. HIGHMORE related the following case:—A lady was prematurely confined at the end of the 7th month of her first pregnancy. She had been complaining for three months previously of uneasy sensations, which she attributed to a fright she received at that time. She had felt the motions of the child up to the time of her labour, which was a very quick one, and was nearly concluded before the practitioner could reach her, the child and placenta being expelled together, on making the first examination. The lady recovered without any bad symptoms. On examining the child, which died during the labour, a rare form of malformation was discovered. The umbilical cord, which was only about five inches in length, passed directly from the navel to the forehead, where the chorion became continuous with the cutis vera of the scalp, which was here perforated, and the dura and pia mater passing into the cord, formed a cul-de-sac and hernia cerebri. The cord then passed on for about one inch to the placenta, in which there was nothing unusual. There were no cranial bones, and the whole of the scalp was occupied by a large nævus. The fœtus was injected with very fine injection, but no vessels could be traced passing to the cord. There were other malformations, but these were of no moment. The fœtus is placed in the museum of St. George's Hospital. Paul Portal has given an engraving of a similar case, and one or two such have been mentioned by German writers. Dr. Simpson, of Edinburgh, had attributed these cases to adhesion, but, in this case, it evidently must have existed from the earliest period of conception, and could be considered only as a malformation.

Mr. COSTELLO said, that the present case of malformation must be referred to some law of nature, with which we were at present unacquainted; it could neither be considered as a specimen of arrest or of excess of development.

Some conversation followed on the often discussed question, as to whether the imagination of the mother had any influence in the production of malformation in her offspring, after which Mr. HALE THOMSON related

AN OBSTINATE CASE OF IRRITABLE BREAST.

E. H., a lady's maid, aged 16, was admitted into the Westminster Hospital on the 29th of August, 1837. The right breast was very large and irritable, and had troubled her for a month previous to her admission. She is a tall, well-grown girl, appearing much older than she really is. Complexion pale and clear. She began to menstruate at eleven years of age, and has been very regular since. The left breast is larger than usual, and flabby. Her general appearance is leucophlegmatic. The affected breast was not, on her admission, very greatly increased, but the glands in

the axilla on that side were slightly enlarged. She complained of the most acute pain running up the neck, and she shrunk away from the approach of a finger. The appetite was bad, and she perspired on the least exertion. She states that the breast became suddenly enlarged in one night, and she attributes it to tight-lacing. On her admission, iodine in all its forms, both locally and internally, was tried without benefit, the pain becoming worse and the swelling increasing. Leeches every other morning, with anodyne fomentations, were next used; then opium, in strong mercurial ointment, was spread thickly over the breast, both without benefit. One-sixth of a grain of extract of belladonna, three times a day, was useless. Iron in all its forms afforded no relief. Half-drachm doses of tincture of cantharides, with half a drachm of spirits of turpentine, and then the cantharides with extract of belladonna, did no good. A strong lotion of hydrocyanic acid, extract of conium, and morphia, and issues, were all equally ineffective, the breast gradually increasing in size, and becoming more painful, and the treatment having continued above six months, she returned home. She now consulted two surgeons of eminence, who employed various remedies without avail. After twelve months of acute suffering, she was again admitted into the hospital in August, 1838. She had now fallen away much in appearance, her countenance was haggard and expressive of the greatest agony. Her appetite was entirely gone, and her strength prostrated. The affected breast was now three times as large as the other, and was exquisitely sensitive over all its surface. A distinct change of structure was perceptible in the mamma, and it appeared hard and unresisting. An issue was formed by means of the potassa fusa, and one ounce of the following mixture was given three times a day; two drachms of tincture of lytta and eight ounces of water. Half a grain of morphia was administered at night. After this the carbonate of soda, and gentian were given, and extract of belladonna externally applied; and, lastly, carbonate of iron in large doses, all without benefit. In consultation it was now determined that a seton should be passed into the upper part of the breast, resting upon the gland itself, and all medicine suspended, even the morphia at night. Two days after the seton the pain in the breast became less, and as soon as suppuration was fairly established the girl gradually improved every day. In a fortnight the affected breast was a very little larger than the other one, and the pain was quite gone. Her countenance has lost its late haggard appearance; she sleeps well; has a good appetite, in fact, appears and feels in perfect health. The glands in the axilla are scarcely to be felt.

Dr. J. Johnson inquired if the pain in the case related was ever remitting or intermitting. It appeared to him, from the history, to be an exquisitely marked case of hypertrophy with neuralgia. The cure was, no doubt, effected by the drain established by the seton. Dr. Graves had detailed some interesting cases of hypertrophy of the mamma with neuralgia, in which the carbonate of iron effected a cure. In all these cases, however, there was an intermission or a remission of the pain.

Mr. Thomson said, that in the case he had related there had been no intermission of the pain; it was always the same, though less in severity, before the treatment he had detailed had been resorted to.

Mr. Chinnock thought much harm had probably been done by the multiplicity of the remedies. He could not conceive what use there could be in the employment of the carbonate of iron, unless there had been intermission of pain. In a case of irritable breast which had been under his care, an issue in the arm afforded immediate and lasting relief.

Mr. Thomson, replied that it was very easy for members to say that remedies were of no benefit after they had been tried and found wanting. The patient under consideration had been seen by men of the greatest experience, who, in similar cases, had administered the same remedies with benefit. The benefit afforded by the seton in this case did not warrant its employment in all instances; indeed, in many cases of diseased breast it would doubtlessly do harm. In three cases of irritable mamma in which he had used setons, harm rather than good resulted. In another instance of enlargement, which took place gradually, counter-irritants of all kinds were used without avail. A seton relieved the disease. In the case just related, there was no sign of disease in the ovaries; there was not, generally speaking, any inflammatory tendency, though in the early stages, on one or two occasions, there was slight increase of temperature, which was removed by cold and leeches.

MEDICAL SOCIETY OF LONDON

Monday, Oct. 29, 1838.

Mr. Bryant, President.

JAUNDICE AND OBSTRUCTIONS TO THE FLOW OF BILE.—THE TREATMENT OF LIVER DISEASE. — THE ADVANTAGES AND DANGER OF MERCURY AND ALKALIES.—VALUE OF ANTISPASMODICS.—NECESSITY OF STUDYING THE REMOTE CAUSES OF DISEASE. — MAGENDIE'S LECTURES ON THE BLOOD.

The discussion of this evening arose out of a question asked by Mr. Crisp, at the last meeting, respecting the effect of a sea-voy-

age on an obstinate case of jaundice, in which he considered that no mechanical obstruction existed.

Dr. J. Johnson said, he could not conceive a case of jaundice in which there was not a mechanical obstruction to the natural flow of the bile in its course to the duodenum.

Dr. Clutterbuck said, another cause of jaundice existed, which, however, might be considered a mechanical one, and that was a spasmodic contraction of the duodenum, preventing the escape of the bile into it. The reason he supposed this cause to be in existence was, the observance of the very sudden manner in which jaundice is occasionally brought on from mental emotion, in persons previously in good health. The symptoms disappeared again in a short time.

Dr. J. Johnson said, the cause assigned by Dr. Clutterbuck was entirely mechanical; the biliary ducts themselves were subject to spasm, by which mechanical obstruction was in a like way produced. What was the best plan of relieving jaundice resulting from the presence of biliary calculi? Have we any power to accelerate the passage of these formations through the ducts? He feared we had not.

Mr. Kingdon considered if we did nothing to aggravate the cause of the stoppage, we could do little to assist in its removal, except, indeed, by mitigating such constitutional symptoms as presented themselves. Regarding the means employed in cases of biliary calculi, he would inquire whether antispasmodics, or those medicines which tended to promote the secretion of the liver, were best. He thought he had seen much injury resulting from the use of opiates.

Mr. Hutchinson was in the habit of employing cupping over the affected part, with much benefit, in cases of jaundice.

Dr. J. Johnson had seen much mischief result from efforts made to remove biliary calculi, by such medicines as turpentine administered in the yolk of an egg, ether, &c. In ordinary cases of biliary calculi, or inspissated bile clogging the ducts, he thought the obstruction was removed by the *vis a tergo* forcing forward the bile. By this proceeding a paroxysm of pain and vomiting was produced (the action might be compared to the parturient efforts of the womb), and after a succession of these attempts, the floodgate gave way; there was a gush of bile, and a relief to the symptoms. If this view, then, was correct, how could we facilitate the proceeding? He considered the vomiting to be an effort of nature to assist in the removal of the obstruction. Generally speaking, however, there was sufficient vomiting present, and emetics were not needed. Small doses of calomel and opium increased the secretion, and, therefore, the *vis a tergo*. These, with saline draughts, to quiet the irritable state of the stomach, and leeches, followed by chamomile fomentations to the affected part, he thought he had seen shorten an attack of jaundice.

Mr. Kingdon considered that Dr. Sangrado's plan, leaving out the bloodletting, would be of as much service as any other. Warm water taken into the stomach would act as the best antispasmodic, and tend to increase the secretion. It would also *encourage* the vomiting; the latter would relax the system, and favour the passage of the obstructing bodies.

Dr. Bennett thought we might do good by favouring the vomiting; vomiting, it would appear, took place when the spasmodic action in the duct occurred. Now he considered that if we could produce vomiting when this irritation was not present, it would be much more likely to favour the passage of the obstructing formations. He thought that quieting the irritation by opiates, and then administering emetics, would be of benefit. One of the most interesting questions the Society could entertain, would, perhaps, be the consideration of the best plan of preventing the recurrence of biliary calculi when they had been once expelled.

Dr. Clutterbuck did not think it difficult to lay down rules as to the mode of proceeding to be adopted, but their successful employment was another matter. He believed the obstruction arose from spasmodic contraction, and that the spasm ceased of itself, after a certain time. He believed some persons were in the habit of passing biliary calculi for a long period of time. These differed in size, from that of a shot to a small pea, and were voided daily, after the patient's suffering from their passage. These bodies could be easily detected on a close examination of the evacuations. As the spasmodic contraction was probably the cause of the obstruction, opiates and the soothing system, with warm bathing, &c. would be beneficial, as tending to relieve the spasm. He thought the best way to prevent the recurrence of biliary calculi, was to alter entirely the action of the liver, and consequently the character of its secretion. It was probable that any change in the action of the liver would produce this effect; thus, occasional vomiting was advisable; frequent mild purgation more so. These, and putting the patient under the influence of mercury, and keeping up the action in a mild way, for a short period, he had seen succeed in preventing a recurrence. If he discovered any signs of inflammation, then moderate bleeding was resorted to; he was not aware of any other means that could be serviceably employed.

Mr. Headland said, that many cases of jaundice existed without pain, the obstruction resulting merely from the presence of inspissated bile. This cause was, to a cer-

tain extent, mechanical. He considered members had not looked far enough for the cause of the obstructions, which must be, he thought, in the blood itself being deteriorated; if so, what was the most useful way of proceeding? All the remedies which had been named were, no doubt, proper remedies, yet could any one of these be said to be a certain cure? Even when mercury was given the practice must be, to a certain extent, empirical; the effect of mercury on the blood was not known. Besides, could mercury be given in these cases without injury? He believed patients with chronic liver affections, who constantly resorted to this remedy, were as frequently killed as cured by it. He thought better remedies than mercurial ones could be employed. Alkalies, it was well known, purified the blood, and rendered it thinner; it was more probable, then, if alkalies or acids were given, the blood might be so influenced in its quality as to cure the disease.

Dr. J. Johnson said, that in persons liable to frequent attacks of jaundice, the bile, if the motions were carefully examined, would be found to be tenacious and ropy. He agreed with Dr. Clutterbuck, that *small* doses of mercury would render the bile more copious, and of a better character; there was no occasion to go into the theory of the formation of bile in arguing on this question. Regarding alkalies, he had employed them for a long time, with those medicines which act on the secretions, such as as iodide of potassium, iodine, nitrate of potash, taraxacum in particular, and small doses of mercury, with the greatest benefit.

Mr. Kingdon believed that the diet, after all, was the chief remedy against the recurrence of biliary calculi. Bile was formed from the blood proceeding towards the liver, in the veins of the intestines, and if veins acted as independent absorbents, as some believed, how necessary was it that the contents of the alimentary canal should be as pure as possible. Regarding the use of alkalies, as they afforded immediate relief, they were great favourites, but the constant use of them was highly dangerous; he had seen purpura and urinary disease result from it.

Dr. J. Johnson thought that the last speaker had an unnecessary *alkaliphobia*. Mischief, it was true, might result from the wholesale use of alkalies, given without any determinate reason; but when acids were present in the stomach, the alkalies did not pass into the circulation, but mixed with the acid. He (Dr. J.) had taken alkalies daily for years, and always carried them about with him; they afforded great relief to acidity, and kept his urine clear; indeed, had it not been for alkalies, he should have been dead long ago.

Mr. Procter considered that small doses of mercury were beneficial in cases of jaundice, by their effect in equalising and improving the quality of the circulating fluid. In anomalous diseases mercury, by producing an alterative effect on the system, produced a cure; he cared not how it acted so long as it led to the latter result. Every practitioner, of course, carefully watched the effect of mercury; he had seen little of the bad effects said to be the results of the use of this medicine.

Mr. Headland would still contend for the necessity of studying more carefully the remote causes of disease, the neglect of which he believed had interfered with our knowledge of remedies; and hence, as a celebrated writer had justly observed, at the expiration of fourteen hundred years, we had knowledge of only two specifics, mercury in syphilis, and sulphur in itch. The nature of the blood was not well understood, and when we looked at the effect of lemon juice in that horrible and loathsome disease, scurvy, we could not too strongly insist on a knowledge of the remote causes of disease. He would refer the members to the valuable lectures by M. Magendie, now publishing in The Lancet, on the blood, for some important facts on the effects of introducing alkalies, &c. into the circulation.

November 5.

A paper on certain improvements made in instruments for extracting teeth, by Mr. Gray, was read.

Mr. Headland alluded to a case in which a woman had been delivered of twins, attached in a manner similar to the Siamese twins. As further particulars are promised at the next meeting, we reserve our report for the present.

SUPPOSED DEATH

FROM AN

OVERDOSE OF MORPHINE.

The following is an abstract of facts contained in a voluminous correspondence between Dr. Jackson, of Dorchester, Dr. Wheelwright, of London, and Messrs. Simpson and Cobb, solicitors, which has been forwarded to us for publication by the latter named gentlemen:—

A lady named Tweed, residing at Dorchester, had been attended by Mr. Arden, a general practitioner, also of Dorchester, from the 21st of April last, to within a few days of the lady's death, which took place rather suddenly, on the morning of Sunday, the 6th of May following. For the last few days of Mrs. Tweed's life Dr. Jackson had

attended, in conjunction with Mr. Arden; the latter-named gentleman, however, having been seized with an attack of gout, Dr. Jackson continued to attend Mrs. Tweed alone, the medicines prescribed being dispensed by Mr. Arden.

On the evening which preceded Mrs. Tweed's death, she was seen by Dr. Jackson, about eight o'clock, who, finding his patient in a very irritable and restless state, considered that an opiate was required. Dr. Jackson immediately repaired to the house of Mr. Arden, and, without writing a formal prescription, mentioned to that gentleman, whom he found sitting up, near the fire, that he should wish to administer to his patient a dose of morphia. Mr. Arden proposed a quarter of a grain of the acetate of morphia; but Dr. Jackson remarked, that the patient had better take "half a grain at once, and have a second draught, also containing half a grain, to take in three or four hours afterwards, if the first did not procure sleep." Mr. Arden immediately sent for the requisite materials, weighed out *one* grain of acetate of morphia, mixed it with twenty drachms of camphor mixture, shaking it carefully together several times, and then divided the whole into two draughts. Mr. Arden, it appears, also wrote the labels for the bottles, but it is not stated whether he corked and prepared them likewise himself.

The first dose of morphia was administered to Mrs. Tweed on the evening of the 5th of May, and the lady expired on the morning of the 6th, about six or seven hours after having taken a single draught. The circumstances under which death took place are not detailed in any part of the correspondence before us. Mr. Henry Lynam, a medical friend of Mr. Tweed's family, asserts that the patient's dissolution was sudden; that she fell into a state of stupor almost immediately after taking the draught, from which she never recovered; and that no pre-existing disease was present sufficient to account for Mrs. Tweed's death. On the other hand, Dr. Jackson affirms to have been informed by Mr. Tweed, that he sat up with his wife until four o'clock in the morning; that she "fell into a calm and quiet sleep," and that he retired to bed, fully satisfied at the time with the effect of the opiate. Hence we conclude that Mrs. Tweed was found dead in her bed. Be this, however, as it may, the unexpected dissolution of Mrs. Tweed seems to have excited the suspicion of her friends, and on the morning of her death the "second draught," supposed to contain half a grain of morphia, was sealed up by Mr. Tweed, transmitted to London, and, after the lapse of some time, examined by Messrs. Battley and Heathfield, who delivered the following certificate:—

"May 25, 1838.

"A bottle of liquor, containing a draught, was examined as follows, viz.:—The liquor (about two ounces) was evaporated to dryness, and two grains of brown, bitter, saline matter were the product; the addition of concentrated nitric acid to this produced a deep red colour, characteristic of morphia.

"For R. Battley,
W. E. Heathfield."

It appears to be clearly established that the bottle was delivered to Messrs. Battley and Heathfield, exactly in the state in which it came from the house of Mr. Arden. The analytical chemists affirm, that it contained *at least* two grains of morphia. Mr. Arden contends that it could not possibly contain more than half a grain of the acetate, a dose which is certainly not too great a one, notwithstanding the opinion of Mr. Phillips, and the authority of his Pharmacopœia. We have thus published, at the request of the friends of Mrs. Tweed, the circumstances attendant upon the death of that lady, so far as we have been enabled to gather them from the correspondence with which we have been furnished. Considering the doubts which seem to exist in the minds of Mr. Tweed and of some of his friends, the proper course to have pursued would have been to have held a coroner's inquest on the body of the deceased; the whole facts of the case might then have been arrived at. For our own part, we may express an opinion that, from the present state of analytical chemistry, Messrs. Battley and Heathfield are more likely to have fallen into error than Mr. Arden.

SUPPLEMENTARY OBSERVATION ON AMAUROSIS.

To the Editor of The Lancet.

Sir:—In discussing, in my paper of last week, the subject of the continental practice—emetics in amaurosis, I forgot to mention a very curious fact, which corroborates my views. Not only, as I have proved, do emetics aggravate congestions in the eye, but, *conversely*, injuries of the retina and ciliary nerves are frequently *accompanied with violent vomiting*. This symptom may attend contusion of the ciliary nerves and retina, even without any wound. This fact is attested by sufficient authority, that of the truly eminent Beer, of Vienna, and its application to the subject of the emetic practice will be obvious to the attentive reader. I have the honour to be, Sir, your obedient servant,

Charles Edwards, A.B., M.R.C.S.
Cheltenham, Nov. 6.

THE HÆMODYNAMOMETER.

THE frequent allusions which are made by M. Magendie, in the course of lectures now publishing in THE LANCET, to M. Poiseuille's instrument, induce us to present our readers with the following description and engravings of the hæmodynamometer. They are derived from the thesis of M. Poiseuille, entitled "Researches on the Aortic Force of the Heart," and published in the year 1828.

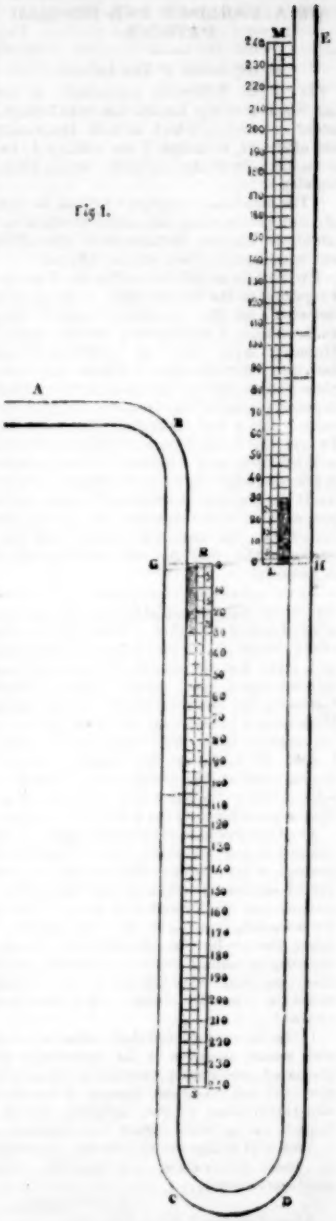
The object of the instrument is, as its name denotes, to measure the force by which the blood is moved through the arteries.

Figure 1 represents the chief portion of the instrument; it presents an horizontal branch, A B; a vertical descending branch, B C; and a third ascending branch, D E. The tube is curved, so as to form a quarter of a circle at A B, and a semicircle at C D. A certain quantity of mercury is placed in the tube G C, D H, and when the instrument is held in a vertical position, the mercury at G is on a level with the mercury at H. Now, if blood be allowed to enter at A B G, through the orifice A, which we may suppose to communicate with the interior of an artery, it will press on the surface of the mercury at G; the metal will descend in the tube G C, and consequently ascend in the tube D E. The degrees of depression and elevation are indicated by the two scales R S and M L, graduated in millimetres, and attached to the vertical branches of the instrument. When the instrument is about to be used, the part, A B G, is filled with a saturated solution of subcarbonate of soda in water. A small brass tube (fig. 2) receives into a concavity

C D, the extremity A of the horizontal branch A B, and is fixed to it with Spanish wax. At the other extremity of this little tube is a screw, which passes into a second brass tube (fig. 3) at G H. The extremity of this latter is pointed, in order that it may readily enter the artery, and is furnished with a prominent rim, L M. As experiments are performed on vessels of different calibre, M. Poiseuille makes use of seven different tubes, the orifices of which, at the extremity, K I, differ from one to seven millimetres in diameter.

The manner of fixing the instrument, and applying it to the vessels of the living animal, have been described by M. Magendie in his lecture in the last number of THE LANCET.

Fig 1.



SIR A. CARLISLE AND HIS MAD PATIENT.

To the Editor of THE LANCET.

SIR:—The following paragraph, in the last Number of THE LANCET has been brought under my notice, which is both inaccurate and offensive, although I am willing to believe, that, in neither respects, was it intentionally so:—

"The assiduous surgeon * * * * at *the hint* of a non-professional Governor, wrote a certificate to consign Holmes, then exhausted, and convalescent, to a lunatic asylum."

The fact is as follows:—On the Tuesday I reported to the Board, that, in going over the wards on the preceding Sunday, as a house-visitor, I had found a patient, named Holmes, in a state of turbulence and delirium; that the other patients had complained they had got no sleep for two nights, in consequence of his violence and constant noise; and I had, therefore, requested Mr. Dasent, the house-surgeon, (whose attention to his duties, and kindness to the patients, is acknowledged by every Governor who attends the hospital), to remove Holmes, under proper care, to the theatre. Sir A. Carlisle stated that the man was *insane*, and dangerous to the other patients, and equally so to himself.

As the admission of an *insane* person is in direct violation of one of the rules of the hospital, I asked whether a certificate to that effect should not be laid before the meeting; and, upon this, the certificate, so much commented upon, was given by Sir Anthony Carlisle; not for the purpose of consigning Holmes to a lunatic asylum, but simply for his removal from the hospital to the workhouse. It is, at *least*, inaccurate, therefore, to characterise the public remark made by a Governor to the committee-meeting, in the discharge of his duty, as a *hint to the surgeon*.

The Governors, individually, ought to feel obliged to any watchful and well-regulated journal, which directs their attention to the useful, and more efficient exercise of their control and performance of their duties at the hospital; but there are two modes of doing this:—By the adoption of the one, the hospital cannot but be benefitted; by the other, injured. Will you allow me to recommend to you the former, and beneficial course?

If the Governors find their voluntary and well meant attention to the institution depreciated, and misrepresented to the public, they will not take that interest in the daily superintendance of the hospital, which I hope to see increase, rather than diminish.

You will oblige me by inserting this letter in your publication. I am, Sir, your obedient servant,

JOHN BICKNELL.

Abingdon-street, Nov. 5th, 1838.

We beg to assure Mr. Bicknell that the word "hint," to which he objects, was not used by us in the offensive sense which it seems to have conveyed to him. We know, from long experience, how necessary is some control over the medical officers attached to public hospitals, and, in default of some more efficient power, we shall ever be ready to second the well meant and judicious attentions of "Governors."—ED. L.

POLITENESS OF OFFICIALS AT THE SMALL-POX HOSPITAL.

To the Editor of THE LANCET.

SIR:—I have been much amused, from time to time, by "Reports" emanating from the Small-Pox Hospital, at Battle Bridge, in which the whole blame of the increased prevalence of small-pox during the last year or two, is laid to the carelessness of parents in not having their children vaccinated, and to the carelessness of medical men in not selecting proper vaccine matter. I shall leave to your readers to judge whether the authors of these "Reports" are not also to blame in a great number of cases, and proceed to give you the following, as a specimen:—

On this very day I went direct from my own house to the hospital, to procure some lymph, and arrived there at exactly half-past twelve o'clock, Dr. Gregory and his assistant (or deputy) were both there. I asked the latter to oblige me with a little matter. He asked me if I had got a lancet, I said I had some old points with me, and produced them. I asked him again, if he could let me have some on the points. He very coldly replied, "if I could wait half an hour *he* would *see* what he could *do*." I told him I could not "wait half an hour" and came away.

Now, Sir, I assert (and challenge contradiction) there was neither press of business nor want of matter, to justify the reply I got. It may be, they are not bound to supply medical men, except when they please, or whether they please or not; but it shows little sympathy with the engagements of their medical brethren, when, in such cases, without inconvenience to themselves, they could save half an hour and do the contrary. I therefore repeat again, they themselves are to blame for the *apathy* they show by such conduct, as, throwing the want of courtesy aside, *it is possible* the two children I intended to vaccinate to day may be exposed to contagion, or infection, before I shall have time to procure some lymph from Mr. Carpue, whose gentlemanly conduct it would cost them little to adopt.

I can also prove they themselves are *careless* as to the *kind* of matter they give, as I, and other medical gentlemen, have very fre-

quently received "purulent matter" in place of "pure vaccine lymph," and refused to use it. I am, Sir, your obedient servant.

W. SIMPSON.

111, Guildford-street,
Nov. 5, 1838.

THE FOTHERGILLIAN PRIZE MEDAL.

WE have received a letter signed *A. Z.*, authenticated privately with the author's name and address, containing the following statement:—A notice was issued by the Medical Society of London, stating that the Fothergillian medal would be awarded in March, 1838, to the author of the best Essay on the Structure, Economy, and Diseases of the Ear. On the Council for estimating the merits of the Essays, was the gentleman to whom the prize was awarded, the "successful competitor" having himself examined the various productions which were presented to the Society by candidates for the prize. The correspondent who makes this statements begs to know if such a proceeding is in accordance with the conditions and regulations established by the founder of the medal, and how often the prize has been obtained by members of the Council, or of the Society.

If the present rule is to be taken for an example, he seems to think that much trouble would be saved to candidates, if the medal were annually awarded to the members of the Council, in rotation, until all have received the decoration, when its distribution beyond the walls of the Society might commence.

AMPUTATION OF THE NECK OF THE UTERUS.

M. RETZIUS, of Stockholm, performed this operation, during the year 1832, three times, but on each occasion the carcinomatous affection returned, and extended to the rest of the uterus. His experience is opposed to that of M. Lisfranc, and from the numerous examinations of the dead body which he has made, he is convinced that carcinoma is rarely confined to the neck of the uterus.

CÆSAREAN OPERATIONS.

A successful example of this operation is recorded by Dr. Wittekop, in "Caspar's Journal," No. 16, 1834. Dr. Wiesel likewise performed the Cæsarean section in the case of a woman, 28 years of age, affected with mollities ossium; the mother died in forty hours, but the life of the child was saved. Dr. Heussner operated on a woman 36 years of age, pregnant for the third time. On her first pregnancy she gave birth to twins, which lived only for a few hours. On the second occasion the labour was extremely difficult. The woman became pregnant seven years afterwards, when the diameter of the pelvis was found not to exceed two inches. A strong, living child was extracted through the abdominal incision. A considerable quantity of blood was lost during the operation, but the patient's strength held up. Symptoms of peritonitis, however, soon supervened, and the patient died on the third day. Mr. Wiesel operated on a woman 30 years of age, whose pelvis was extremely deformed; the conjugate diameter not exceeding one inch and three-quarters. The child was brought away alive, but died soon after birth; the mother also died, with symptoms of gangrene of the uterus, on the eighth day. M. Blei, under similar circumstances, was fortunate enough to save the life of the child; the mother, however, died on the fifth day after the operation.

The Cæsarean operation has been frequently had recourse to recently, *after* the mother's death. M. Haak operated after the lapse of half an hour; Dr. Held after one hour and a half; Dr. Schild after eight hours; but in all these cases the children were extracted dead.—*Siebold's Journal of Midwifery.*

TO CORRESPONDENTS.

THE LATE MR. LOCKLEY.—Next week we shall extract from the daily journals, some letters which have appeared on the subject of the conduct of Sir Henry Halford towards his friend, Mr. Lockley, and add to them a few particulars relating thereto which have not yet been published.

The letter of *Mr. D. O. Edwards*, disclaiming a participation in any strictures which may be supposed to be contained in the last number of THE LANCET on the "Anglican Church," has been received. We freely exonerate Mr. Edwards from the charge to which he seems to think he may be subjected by those friends who know him as an occasional contributor to our columns from the Westminster Hospital; but, at the same time, we are at a loss to discover reasonable grounds for his apprehension. However, certain it is, that Mr. Edwards neither wrote, nor aided in writing, one line of the contents of the last number of this Journal.

J. T. L. Attendance on the lectures alluded to will be received at Glasgow, but not at Edinburgh.

Catharine K. We cannot give publicity to vague charges against a medical practitioner, unsupported by the slightest shadow of proof.

The communications of *Messrs. Curtis, Hytch*, and *Froggult* have been received.